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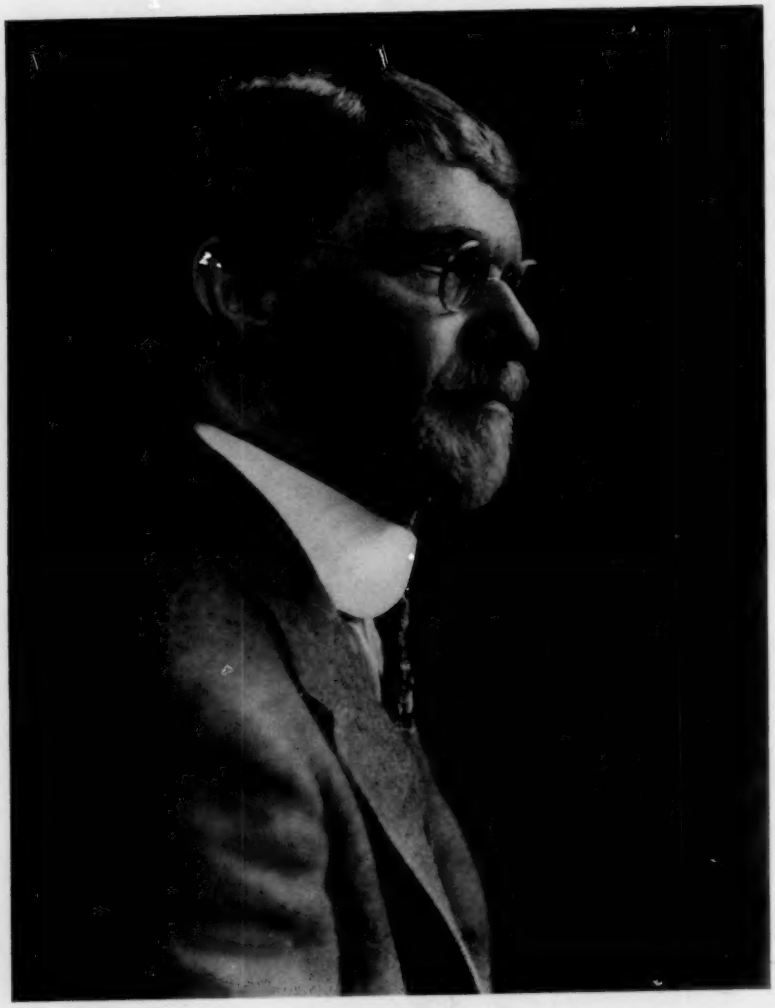

HOWARD CROSBY WARREN

1867–1934

VOL. 41, NO. 2 MARCH, 1934

THE PSYCHOLOGICAL REVIEW

HOWARD CROSBY WARREN

ASSOCIATE EDITOR, PSYCHOLOGICAL REVIEW, 1901–1909
EDITOR, 1916–1934

Quite suddenly and unexpectedly our highly esteemed and universally beloved colleague has terminated his forty years of manifold service to the advancement of psychology. No one during this formative period has labored more effectively for the organization of psychologists and the facilitation of their research, and for the recognition of psychology as a science in its own right, deserving of independent academic status. The development of his own laboratory from its small beginnings in the '90s to its present splendid condition is no mean contribution. As editor and publisher of psychological journals he has served the common good unstintedly. His contribution to the adequate bibliographic facilities of our science began early and were long continued. Always an active member of the American Psychological Association, he held the office of President in 1913, and for many years was chairman of important committees.

A special line of interest was that of the improvement and standardization of psychological terminology. He saw very clearly the need for agreement on fundamental definitions and for system and logic in the assemblage of terms. His open-mindedness and judicial temper fitted him for leadership in this field where opinions often clashed. For years he collaborated in this enterprise with other members of the Association, and finally, by taking the responsibility on his own shoulders, was able to enlist wide coöperation in the arduous

task of preparing a 'Dictionary of Psychology' that should be as nearly as possible complete and authoritative. This important work he left so nearly finished that there is no doubt of its early publication. It will be a fitting monument to his memory.

Warren's contributions to psychological theory are very substantial. His disposition was to look fearlessly at both sides of a question, and not to sacrifice the merits of either side to the claims of the other. As a youth, he tells us, he was inclined towards mysticism and the occult but at the same time convinced of natural law and the uniformity of nature. He worked through to a thoroughgoing mechanistic conception which appeared the best working hypothesis for science; but mental phenomena seemed to him undeniably real. The double-aspect theory of the mind-body relation appealed to him because it provided a place for consciousness in the real world, without infringing on the orderly mechanism of nature.

With the advent of behaviorism, Warren's reaction was interesting and characteristic. He was much impressed with the value of the behavior method and entirely hospitable to its positive findings; but they did not blind him to the value of introspection as a method nor to the significance of some of the older conceptions. For stimulus and response he preferred the terms impression and expression, because they brought the organism itself into the picture. The organismic conception was quite acceptable to him, and he pointed out its antecedents in pre-behavioristic psychology; but its value did not blind him to the existence of *parts* within the unitary organism. The organism is not an amorphous whole but an articulated system; and moreover it is imbedded in the environment and its activity consists in effecting adjustments with the environment. It is the task of psychology to examine this process of adjustment by all available methods, by introspection, by behavior study and especially by neural investigation.

Purpose appeared to him as a sure fact of conscious experience and also, in the form of anticipatory reaction, a sure fact of animal behavior, of individual growth and of phy-

logeny. But purpose does not imply entelechy nor any limitation on the universality of mechanism in the physicochemical processes of life. The notions of *compulsion* and of *potency* are both to be excluded as anthropomorphic and even 'psychomorphic' from any general theory of organic activity—as they have no place, either, in physical theory. "It is in fact an anthropomorphic interpretation of natural phenomena to conceive of the antecedent terms in a causal series as exerting compulsion upon subsequent events. The transformation of a quantity of hydrogen and a quantity of oxygen into water is due to their nature and to circumstances in the environment." Therefore we need not be afraid to admit anticipation in growth and in behavior, nor to speak of the outcome of an orderly course of action as a determining (not compelling) factor in the preparatory stages of the action. This discussion needs a much fuller account to show its full importance.

Warren was a lifelong student of associationism, and his 'History of the Association Psychology' is a worthy fruit of this study. It is an important contribution to the history of psychology, just because the associationist school has been so fully considered by the historians of philosophy, who however have evaluated the members of the school mostly by their philosophical contributions, and who have accordingly dismissed Hartley and others as 'mere psychologists.' Warren's own associationistic views were broadened gradually, he tells us, to include evolutionary and biological principles.

Though in a sense eclectic, Warren was by no means a weak-kneed devotee or compromise. His hope was by thorough examination of fundamentals "to reveal a middle ground on which the two opposing parties could unite." Undoubtedly this stand for mutual understanding has been an important influence in maintaining the solidarity of our science during the past generation.

R. S. WOODWORTH

Columbia University

THE CONCEPTUAL FRAMEWORK OF TOLMAN'S PURPOSIVE BEHAVIORISM

BY STEPHEN C. PEPPER

University of California

I. The Revolutionary Character of Tolman's Concepts

In the last paragraph of his 'Purposive Behavior in Animals and Men,'[1] Tolman asks the philosopher: "We are asserting, are we not, a pragmatism?" If I may be 'the philosopher,' let me answer, "You are, but with certain curious inheritances still holding over from the traditional mechanistic psychology of the past." Tolman is so much aware of the revolutionary concepts he is employing in psychology, of the *bouleversement complet* he is performing, that it is a matter of more than usual interest to examine the structure of his method. Moreover, I am sometimes not sure that he himself realizes how *complet* is his *bouleversement.*

Some scientific revolutions are due simply to the discovery of new facts which alter the interpretations of large fields of old facts without altering fundamental modes of interpretation. Other scientific revolutions are due to the strain of great masses of facts which are somewhat inadequately interpreted in an old set of fundamental concepts, and which finally burst through the old set and demand a set wholly new. In the former sort of revolution the categories of interpretation remain the same; only an hypothesis about the facts changes. But in this latter sort of revolution the categories of interpretation themselves change, and the scientist is called upon to reconstruct, so to speak, his whole perceptual apparatus. I say 'perceptual,' and not 'conceptual,' advisedly, for it is the very look of facts that undergoes change. The sort of revolution proposed by Tolman in his 'Purposive Behaviorism' is, I believe, of this latter type.

[1] E. C. Tolman, Purposive behavior in animals and men, New York, Century Co., 1932, xiv + 463.

The psychology of the last two centuries has, with few deviations, been developed under the philosophic categories of mechanistic naturalism. From John Locke to John Watson there have been many varieties of psychology, but nearly all of them have been of one type. They have been species of a single genus. They have been hypotheses under a single categorial mode of interpretation. Lately this mode of interpretation itself has been subject to strain from various quarters. The explosion may be said to have occurred with the Gestalt psychologists. But, to my knowledge, Tolman's 'Purposive Behavior in Animals and Men' is the first work that has gathered up the fragments and put them together in a new framework. The symptom of this constructive change and of its depth of reorganization is the large and forbidding vocabulary of new terms, which the work contains. Tolman found it necessary to append to his book a glossary of one hundred and twenty-four terms for the assistance of his readers. These are not new terms for old concepts. They are terms for new concepts. The reader of 'Purposive Behavior' is invited to learn a new vocabulary and to reconstruct his mind for the understanding of the book. The new concepts are those of pragmatism, or, to avoid certain unhappy associations with that name, those of contextualism.

To one immersed in the present philosophical stream of contextualism, Tolman's work appears like an unexpected confirmation. The contextualistic philosophers working on the traditional material have gradually been systematizing their concepts, and are now beginning to see rather clearly how these concepts are interconnected, and how they affect the facts. A set of categories, a fundamental mode of interpretation, has now been more or less clearly worked out, and the neatness with which Tolman's analyses fit into these categories comes as a verification of the latter.

Yet no great change in human affairs is ever made without transitions. Before the change is completed, something of the old sticks to the new that does not really belong to the new, and something is not yet added to the new that really belongs there. The early automobiles had whip sockets and lacked

windshields. It may perhaps be of value to examine Tolman's work in the light of a set of contextualistic categories and to estimate how much remains in it that belongs really to other modes of interpretation, and how much it lacks of a full contextualistic interpretation.

II. Contextualistic Concepts Contrasted with Mechanistic Concepts

Let me begin by briefly exhibiting the contrast between mechanism and contextualism.

The main categories of mechanism are: first, a *spatio-temporal field;* second, *primary qualities* qualifying locations in that field; third, *natural laws* determining the configurations of locations qualified by *primary qualities;* fourth, *secondary qualities;* fifth, *laws determining the correlations* of secondary qualities with configurations of primary qualities; and, sixth, *laws determining* the sequences of *secondary qualities.*

In this simple mechanistic interpretation the natural sciences, physics, chemistry, biology, are considered as dealing with the first three categories only. The secondary qualities appear as an unnecessary encumbrance upon the universe, and, if acknowledged, are relegated to the somewhat dubious science of introspective psychology and the 'speculations' of philosophy. They may be ignored, however, or even denied after the manner of J. B. Watson, in which case the last three categories drop out of the picture, and psychology becomes a somewhat inconspicuous branch of biology, specifically a branch of physiology. Psychology so considered is physiological behaviorism. As Tolman points out in Chapter I, even Watson is unable to hold consistently to this interpretation of psychology. But if the secondary qualities are acknowledged, the science that deals with them is introspective psychology.

In spite of the great apparent difference between them, introspective psychology and physiological behaviorism are in their structural features identical. Both are dominated by the method of element analysis. For both, explanation consists in exhibiting the parts out of which a whole is composed.

A whole is nothing but an aggregation of parts, or at most a spatial configuration. Name the parts and their positions relative to one another, and state the laws by which these parts moved into their present positions from previous positions, and the task of description is completed. Whether the parts be sensations, images, and feelings governed by laws of association, or physiological reactions governed by a law of conditioned reflex, makes no fundamental difference in the mode of conception. Both are atomic modes of description, giving dominance to simple external relations of parts in an aggregate, of time sequence, and of spatial position. In the *last* analysis every explanation goes back to a physical description of an event in terms of certain positions in space and time of certain ultimate physical elements. Physics is, consequently, the fundamental science. All other sciences deal merely with large, unanalyzed aggregates, and rough correlations which await adequate descriptions at some time when these crude wholes can be broken down into their physical elements.

The main categories of contextualism are: (1) *texture*, (2) in an *environment* of other textures, (3) analyzable into *strands* which extend into environmental textures, and (4) which have *references* (sense of direction) to or from environmental textures or (5) towards *consummations* yet to come, or (6) from *initiations* gone by. The (7) *quality* of a texture is a (8) *fusion* of its strands and there is no quality more fundamental than this felt or observed quality of the total texture

From these categories many consequences follow. But I shall only point out those which exhibit a marked contrast with features of a mechanism. First, these categories imply a denial of the legitimacy of atomism. The most elementary thing, in the sense of that behind or beneath which you cannot go, is a texture. A texture by its very nature is a complex whole having what are technically called internal relations. That is to say, the parts of the whole can be adequately described only as parts of the *whole*. The character of the whole enters into its parts, and unless the function of a part in the whole is exhibited in the description of the part, the part is

no longer a part of that whole; it is a part of some other whole. The term 'strand' suggests this condition. A strand is relative to a texture. If you follow a strand out of its original texture, thinking thereby to find it isolated from all textures, you deceive yourself; for you will only discover that the strand has entered into a new texture with reference to which it has a new function, and consequently, according to the seventh category, a new quality.

If you hear a pitch in a tune and then think you can isolate that pitch by showing its place in a muscial scale, you deceive yourself. You have simply traced the pitch of a tune to a pitch in a musical scale. The scale is as much a whole, a texture, in its own right as the tune. The two textures are different textures and qualify the pitch differently. The pitch has a different function in the two textures. Or take the pitch out of both scale and tune, and hear it continuously in a surrounding silence; you have still not got it out of a texture. The silence itself is a ground against which the pitch is heard, and the substance of the silence is likely to be an emotional tension of even greater intricacy and variability than the textures of a scale or a tune. In fact, the quality of a so-called isolated pitch is less controllable than that of a pitch in a tune or a scale—as anyone who has tried to perform 'analytic' experiments in esthetics has discovered to his grief. Control is obtained by developing a texture sufficiently complex to guarantee a specific function to a given nameable strand. The quality of a pitch in a tune or a scale is pretty well controlled, but not in a so-called silence. A part, then, can never be described in isolation. A whole composed of isolable parts is an illusion. Element analysis, in the sense of breaking a thing down to isolated elements, is consequently impossible.

What then do we do, when we think we analyze a whole? The answer to this will be our second point. What we actually do is to trace the strands of the given texture into some other texture or set of textures which have proved to be convenient instruments of control. A texture employed as an instrument of control in this manner may be called a schematic

texture, or simply a *scheme*. A musical scale is a good example of a scheme. Any piece of European music can be analyzed into notes on a staff. If still more accuracy of control is desired, these notes may be referred to a scheme of air vibrations, which fits into a system of schemes called physics. These schemes are our most valuable instruments of knowledge, and the more closely these schemes can be interknit, the more thoroughly correlated our knowledge. But it must not be imagined that these schemes give the substance of what they control. They are only sets of references suggesting operations by means of which men can obtain textures of some desired character. A musician with the scheme of a musical score before him can follow the references there suggested to the keys of a pianoforte keyboard and produce a texture of rich quality. An engineer following the references of physical formulæ does the same thing in the construction of an aeroplane. But no contextualist would dream of identifying the texture of the musical score with the texture of the music or of thinking that because the notes of the musical score were little dots with hard outlines around them, therefore the tones of the music must have a similar nature; and no contextualist would think of identifying the structure of physical formulæ with the structure of an aeroplane.

Analysis is an operational affair, a matter of following references from one texture to another. There is no assumption of similarity between the analyzed texture and the analyzing texture. If you call the latter texture the idea of an object, and the former texture the object, you have the type of all knowledge for a contextualist. The idea of an object is an instrument that will guide you to the object. The idea symbolizes the object in that sense. It does not need to copy the object in any manner whatever. To know an object does not mean that you have a picture of it. It means simply that you have an instrument by means of which you can obtain the object.

Thirdly, one should notice that there is no 'inner' or 'outer' in contextualism such as develops in mechanisms from the distinction between secondary and primary qualities and

the consequent distinctions between mind and matter. There are different textures, and the character or quality of one texture is different from that of another, but the strands of one texture can be followed into another. Textures are freely communicating, and all is above board. Perception is not an 'inner' idea corresponding in some puzzling way with an 'outer' object; it is a traceable relation of strands in a texture. Desire is not an 'inner' idea qualified by an 'inner' feeling and attaching itself to an 'inner' will, which in some manner is projected into 'outer' motions directed upon an 'outer' object. Desire is a texture of strands with references which can be followed to a terminus producing quiescence or satisfaction. The issue between introspectionism and behaviorism, as this issue was stated a decade ago, vanishes.

III. Tolman's Immanent Determinants

On the basis of these remarks, let us now observe the extent to which Tolman is a contextualist. The main features of his conceptual structure can be found in Chapters I and II. In the first few sections of Chapter I, Tolman distinguishes between 'molecular' and 'molar' behaviorism, contending that only the latter is the psychologists' concern, and that moreover there are properties in 'molar' behavior which are not reducible to 'molecular' physiological reactions. "An act *qua* 'behavior' has distinctive properties all its own. These are to be identified and described irrespective of whatever muscular, glandular or neural processes underlie them. . . . A rat running a maze; . . . my friend and I telling one another our thoughts and feelings—*these are behaviors* (qua *molar*). And it must be noted that in mentioning no one of them have we referred to, or, we blush to confess it, for the most part even known, what were the exact muscles and glands, sensory nerves, and motor nerves involved. For these responses somehow had other sufficiently identifying properties of their own" (p. 8).

He then describes the identifying properties. They consist of two sets; (1) initiating causes, and (2) behavior determinants. There are two sorts of initiating causes, viz.

(*a*) 'environmental stimuli' and (*b*) 'initiating physiological states.' There are three kinds of behavior determinants, viz. (*a*) purposive and cognitive immanent determinants, (*b*) purposive and cognitive capacities, and (*c*) behavior adjustments. By 'behavior adjustments' Tolman means his 'behavioristic substitute for, or definition of, what the mentalists would call conscious awareness or ideas.'

Of these factors, we will select first only the immanent determinants for consideration. These turn out to be the core of a behavior act. These immanent determinants, it is, that carry an act of behavior along. The act is set off by environmental stimuli and physiological states; it is made possible by certain capacities in the organism; and, on occasion, it is assisted by 'behavior adjustments.' But what actually carries an instance of behavior through to its termination is the 'purposive and cognitive immanent determinants.' What specifically are these?

In paragraph 8 of Chapter I, Tolman gives the following definition of purpose: "The doctrine we here contend for is, in short, that wherever a response shows docility relative to some end—wherever a response is ready (*a*) to break out into trial and error and (*b*) to select gradually, or suddenly, the more efficient of such trials and errors with respect to getting to that end—such a response expresses and defines something which, for convenience, we name as a purpose." Since this definition seems to make the immanent purpose dependent upon trial and error learning, and since subsequently Tolman describes other types of learning in which purpose is nevertheless present, it is well to add to this definition the one given in the Glossary: "A purpose is a demand (q.v.) to get to or from a given type of goal object. Such a purpose is testified to objectively by the fact that behavior tends to persist to or from and to show docility (q.v.) relative to getting to or from specific types of goal object (q.v.) or goal situation." This second definition makes a distinction between a purpose and the evidence for it, which is a very important distinction and which the first definition blurred. A purpose, as a structural concept, apart from matters of evidence, is simply 'a demand (q.v.) to get to or from a given type of goal object.'

But fully to understand this definition we must follow the '(q.v.)' indicated after 'demand.' We learn that a 'demand' is 'an innate or acquired urge to get to or from some given instance or type of environmental presence or physiological quiescence or disturbance.' A purpose, then, in the last analysis turns out to be a demand, and its ultimate goal is the attainment of, or escape from, some sort of object; or the attainment of, or escape from, some sort of physiological state. An object is, of course, a different sort of thing from a physiological state, but we shall let that difference lie since it will automatically clear itself up in the later analysis. So much for the purposive immanent determinant.

Now, to the cognitive immanent determinant. In paragraph 9 of Chapter I, Tolman describes this factor as a complex of three assertions or judgments: "Our contention will be that the characteristic patterns of preferred routes and of commerces-with which identify any given behavior-act can be shown to be docile relative to, and may *pari passu* be said cognitively to assert: (*a*) the character of a goal object, (*b*) this goal-object's initial 'position' (*i.e.*, direction and distance) relative to actual and possible means-objects, and (*c*) the characters of the specifically presented means-objects as capable of supporting such and such commerces-with."

That is to say, implied in the behavior act and essential to an adequate description of it, are three distinguishable sets of references: one set to the goal object, one set to the 'position' of the goal-object and also to the 'positions' of means-objects, and one set to the means-objects. It should be noticed that 'position' is written in quotation marks and is not to be interpreted as literal spatial position. It is a special kind of intermediate reference. Literal spatial position is simply one mode in which this special kind of reference exhibits itself, and consequently something less fundamental than the type of reference and derivative from it. This point is so important that I will quote the paragraph in full, in which Tolman calls explicit attention to it. "It should be observed," he writes, "that we are here using the terms *distance* and *direction* (the two elements of 'position') in a

special and somewhat arbitrary sense. By distance we are covering the general situation whereby a given object affords (or does not afford) a relatively 'short' way of getting to a demanded goal. And by direction we are meaning not merely differences of spatial pointing, such as right-turning-ness as against left-turning-ness and the like, but also differences in the general characters or qualities of the object, as, for example, its whiteness vs. its blackness, its alley-like character vs. its crevice-like character, and the like. By distance we are going to mean all kinds of 'goodness' and 'badness' for getting-on—spatial, temporal, gravitational, physiological, rhetorical, social, or what not. And by direction we are going to mean all kinds of qualitative characterization of the given to-be-selected means-object, irrespective of whether this characterization really be one of spatial direction, or one, rather, of color, size, weight, type of muscular activity, type of verbal phrase, type of social performance, or what not" (Chapter VI, paragraph 3).

There are no words in the English language (or any language) with which to specify these new concepts. Accordingly, Tolman takes the words naively employed for these concepts in simple maze experiments, and generalizes them for his novel uses. 'Distance' and 'direction' are two forms of a special type of cognitive reference. This type of reference is a fundamental, a categorial, concept. It can only be observed, denoted, and named. It cannot be further explained. Above all, one must carefully avoid interpreting it in spatial mechanistic terms. It is a cognitive reference; and it is not feet and inches (except as these are interpreted as a cognitive reference).

After this important digression, we return to Tolman's description of the cognitive immanent determinant. So far, we have found it to consist of three sets of references. And so far as anything Tolman says up to this point or for the next one hundred and twenty pages, these three sets of references might be regarded as more or less separate one from another. From Chapter II through Chapter VIII, Tolman is simply proving the existence of the various concepts he has defined in

his first chapter, and exhibiting a certain amount of relevant detail that comes to light in the process. But in Chapter IX, something new happens. Here we are introduced to the concept of the 'sign-gestalt.' For here it is that Tolman first comes to grips with the various types of *complete* thought process. Up to this point he has merely sought to establish the existence of the factors he wishes to use in his description. Here for the first time, he is faced with the problem of giving descriptions of total thought processes—of perceptions, mnemonizations, memories, and inferences. To handle these total situations, he finds himself forced to introduce the new concept of sign-gestalt.

In principle, the concept of the sign-gestalt is nothing more nor less than the assertion of the mutual interconnection and 'fusion' of the three sets of cognitive references into a single functioning system of references which as a whole give the behavior act its structure. Learning consists in the perfecting of such a behavior structure. And there is no act of behavior, however tentative, that does not contain the rudiments of such a structure. Tolman points out and insists that a stimulus and act are not to be thought of on the analogy of a ball and nine-pins but rather on the analogy of a move in chess. "The honk of an automobile horn will 'set' all save a new-born infant, not only for that sound *qua* sound or even for the type of horn which produces it, but also for the whole signified complex: automobile plus impatient driver" (Ch. IX, paragraph 6). And again, more abstractly, "The connection should be indicated between this concept of sign-gestalts and that of direction-distance correlation elaborated in the preceding chapters. Obviously the two concepts are closely related. In fact, we shall now assert that what we are here calling sign-gestalts are in fact no more than direction-distance correlations with, however, a further emphasis upon their whole-like or gestalted character. In short, that which we formerly designated as the direction of a means-object we are now calling an immediately presented (or a ready-for) 'sign object.' And that which we previously designated as the correlated 'distance' we are now calling an expected (or

ready for) 'shortness' or 'longness' of the 'signified means-end-relation' between such sign-object and some resulting significate. The concept sign-gestalt is, however, more inclusive and more useful in two ways: (*a*) The sign-gestalt concept emphasizes that the distance is always a distance to a significate (possible goal-object) as well as from a sign; and (*b*) the sign-gestalt concept also emphasizes that the sign-object (*i.e.*, the means-object of such and such a 'direction') plus signified means-end-relation (*i.e.*, expected distance) plus signified object (*i.e.* expected goal-object or possible goal-object) never actually function in behavior as separated or atomized units. They always fuse together with some single gestalt-like whole" (Ch. IX, paragraph 10).

In other words, the three sets of references of the cognitive factor in the immanent determinant, are intrinsically a system. Each may be separately emphasized for the sake of explanatory clarity, but 'in fact' all three function inseparably as a whole. If you want to know what behavior really is, you must synthesize these three sets of references into a sign-gestalt. A sign-gestalt is not explained in terms of the sets of references into which it can be analyzed, but the reverse. The abstract sets of references are explained by their synthesis in the sign-gestalt, within which they function.

This synthetic process of explanation does not stop here. The next step is to bring the purposive factor of the immanent determinant and the cognitive factor together in a totally functioning whole. For it must be obvious that a set of references, however fully synthesized in a sign-gestalt, cannot function without the dynamic character of the purposive factor in the immanent determinant exerting its influence. A pattern of references simply remains a pattern of references without action, unless it is put into action. In fact, in concrete behavior, such a pattern always is in action. More than that, the references themselves are inconceivable except in terms of incipient action. They are as Tolman shows over and over again 'expectations.' The cognitive factor presupposes the purposive factor. In like manner, the purposive factor, with its inherent reference to a goal-object, pre-

supposes the cognitive factor. Just as the different sets of cognitive references in the cognitive factor continuously entail one another, however much in the course of the explanatory exposition of one of them the others might be apparently ignored; so the cognitive factor as a whole entails the purposive factor as a whole, and vice versa, however much each may be separately considered in exposition to the apparent neglect of the other.

The synthesis of these two factors of the immanent determinant produces what Tolman calls (*a*) first order drives, (*b*) second order drives, and (*c*) personality mechanisms. Since these are all alike in general structure, we shall not need to pay much attention to their distinguishing characteristics. In fact, these distinguishing characteristics do not seem to be very clearly made out. Tolman himself does not pretend that they are. I shall, therefore, take the liberty of calling them all 'appetites' and 'aversions,' even though this procedure puts some strain on Tolman's use of these terms.

For the most part Tolman identifies 'appetites' and 'aversions' with first order drives, which are fundamental innate impulses to action. Nevertheless, he discovers (Chapter XIX, paragraph 5) that second order drives are all 'aversions.' Like first order drives they are innate. They differ from first order drives only in being instrumental and not self-sufficient. Second order drives are innate drives that are instrumental to first order drives. Personality mechanisms are acquired in various ways on the basis of first and second order drives. Their structure, however, though not their history, seems to be essentially the same as first or second order drives. A phobia could aptly be called an acquired aversion, and a sex perversion could aptly be called an acquired appetite. Indeed, since innate drives generally require a certain amount of training for their complete and perfect functioning, a training that requires not only a development of muscular skill but also a refinement of discrimination for suitable stimuli, one cannot help feeling that the distinction, actually employed by Tolman, between innate drives and personality mechanisms is not that of innate impulsive behavior versus acquired im-

pulsive behavior, but that of normal versus abnormal development of behavior. Of course, the determination of whether behavior is normal or abnormal is something that lies outside of the structure of a behavior act. It appears to me, therefore, that the examples of behavior adduced by Tolman under the three headings, first order drives, second order drives, and personality mechanisms, could well be gathered together as various kinds, or stages of development of, appetites and aversions. The general structure of a behavior act in all these cases seems to be the same, and that structure is an organicity of all the immanent determinants, the purposive as well as the cognitive. In other words, an act of appetition or aversion is one in which the functions of the purposive impulses are included with those of the cognitive references in the description of a total behavior act.

IV. The Contextualistic Nature of the Immanent Determinants

Appetites and aversions thus present us, according to Tolman's descriptions, with total behavior acts. Here, then, is perhaps a good place to stop and consider to what degree Tolman has already shown himself to be a contextualist. Of the contextualistic categories—(1) synthetic texture, (2) environment of textures, (3) strands, (4) references, (5) consummations, (6) initiations, (7) quality, (8) fusion—Tolman's description of an act of behavior clearly accepts all but the second and the seventh. His behavior act is a synthetic whole containing strands with purposive and cognitive references producing demands and attaining consummations after definite initiations, and all (in his own words) 'fused together.' He not only accepts these six out of the eight contextualistic categories; he refines them and develops them beyond what any contextualists have been able to do up to the present time. Why does he not accept the remaining two categories which are integral to the set?

It is not certain that he does not accept the second category—that of an environment of textures. Of course, he accepts some sort of environment for his behavior-acts.

but it is not clear what sort of environment. He too often leads one to believe that he is conceiving of the environment of a behavior-act in strictly mechanistic terms—an environment of electro-magnetic vibrations and chemical atoms. Not that the contrast between synthetic act and atomic environment is ever exhibited in such an absurd juxtaposition, but that continually the mechanistic mode of thought seems to make itself felt when he is dealing with environmental factors in a way that does not appear when he is dealing with factors in the behavior-act itself. It goes without saying that such a categorical shift of conceptions as this, from treating an act as molar to the treating the act's environment as molecular, is not only absurd but confusing and methodologically illegitimate. There is every reason to think—especially as Tolman makes no explicit statement to the contrary—that he would see the advantage of conceiving the nature of the environment as categorially of the same type as the nature of the act. If he did so, he would thereby accept the second category.

But his attitude toward the seventh category is unambiguous and truculent. He will have nothing, absolutely nothing, to do with qualities. Or rather, I should say that this is his avowed attitude, for actually he is employing qualities on almost every page of his book. His book accepts the seventh category, but he himself will not. Whence this contradiction? Obviously, the denial of qualities is an inheritance from the mechanistic behaviorism on which Tolman was nourished, whereas the implicit acceptance of them is an insight into the needs of the contextualistic behaviorism into which he has grown. Yet the contradiction is not a flat contradiction. For the mechanistic qualities he is denying are the atomic qualities of structural psychology, whereas the contextualistic qualities he is implicitly affirming are the synthetic qualities of gestalt psychology.

His difficulty arises from identifying qualities with the atomic secondary qualities of mechanism. These qualities, according to the mechanistic categories, are private, inner, mental entities contrasted with the primary qualities which

are the objective, outer, physical entities. To increase the contrast still further, recent mechanistic theorizing has resolved the primary qualities entirely into relations, so that all qualities become mental, inner, private, subjective. The typical mechanistic dichotomy of matter and mind results. It seemed to follow that each of these 'substances' required separate study. So, mechanism ascribed certain objective methods of investigation to matter, and certain introspective methods to mind. Physiology thus became detached from introspective psychology, and the problem arose how to connect them. This is a typical mechanistic problem and does not arise at all within the contextualistic categories. The various 'scrap-heaps' for 'raw feels,' which Tolman so whimsically describes in the last pages of his book, are nothing more nor less than the typical mechanistic attempts to bridge the gap between mind and matter. They are, consequently, entirely irrelevant to the main swing of his book.

A mechanistic behaviorism of the Watsonian type would be compelled to meet the problem of qualities versus physical relations. Such a behaviorism ignores qualities in its more or less atomic physiological descriptions, and sooner or later must come to grips with the problem of what happens to these qualities. Tolman, as a result of his Watsonian inheritance, imagines that he also has been ignoring qualities, and therefore thinks that he also must face this problem. "'Raw feels'," he writes, "are our naturalistic 'map-name' for a side of mind which our map ignores, or reduces to impotency. But, it may be asked, having thus reduced the richness of immediate reality to 'raw feels,' what more specifically (to mix our figures) is the precise scrap-heap into which our map then chucks such 'raw-feels'?" (p. 426). The truth is Tolman never has ignored these 'raw-feels' or qualities. But, because they were synthetic contextualistic 'raw-feels' and not atomic mechanistic 'raw-feels,' he failed to recognize them for what they were, though almost every page of his book is spread over with them.

Let me show how this is the case. Suppose we go to the

very beginning of the book where Tolman is explaining his conception of the 'molar definition of behavior.' Here is what he says: "An act *qua* behavior has distinctive properties all its own. These are to be identified and described irrespective of whatever muscular, glandular, or neural processes underlie them. These new properties, thus distinctive of molar behavior, are presumably strictly correlated with, and, if you will, dependent upon, physiological motions. But descriptively and per se they are other than those motions" (p. 8). These properties, in other words, are not atomic but synthetic. What are they? "The first item . . . is to be found in the fact that behavior, which is behavior in our sense, always seems to have the character of getting-to or getting-from a specific goal object, or goal situation. . . . As a second descriptive feature of a behavior-act we note the further fact that such a getting-to or from is characterized not only by the character of the goal-object and its persistence to or from it, but also by the fact that it always involves a specific pattern of commerce-, intercourse-, engagement-, communion-with such and such intervening means objects, as the way to get to or from. For example, the rat's running is a getting to food which expresses itself in terms of a specific pattern of running, and of running in some alleys rather than in others. . . . Or, the man's behavior is not merely that of getting from his office to his be-wife-ed or be-pantry-ed home; it is also the doing so by means of such and such a specific pattern of commerce with the means-object—automobile, roads, etc. . . . As the third descriptive feature of behavior-acts, we find that, in the service of such gettings to or from specific goal-objects by means of commerces with such and such means-objects, behavior acts are to be characterized, also, in terms of a *selectively greater readiness* for *short* (or easy) means activities as against *long* ones" (pp. 10–11).

Now, I ask, what are these molar characters in terms of which 'behavior, which is behavior in *our* sense' is described—'getting to,' 'getting from,' 'character of goal object,' 'means object,' 'getting to food,' 'specific pattern of running,'

'running in some alleys,' 'getting from his office,' 'be-wife-ed and be-pantry-ed home,' 'automobile,' 'roads,' 'short (or easy),' and 'long,'—what are these but qualities? Synthetic qualities, to be sure, but nonetheless—on the contrary, all the more—qualities.

Tell me, if you can, that the following 'objective' description of the behavior of Tinklepaugh's monkey is not qualitative: "The experimenter displays a piece of banana, lowers the board and places the banana under one of the cups. The board is then raised, and working behind it with his hands hidden from the view of the monkey, *the experimenter takes the banana out and deposits a piece of lettuce in its place.* After the delay, the monkey is told to 'come get the food.' She jumps down from the chair, rushes to the proper container and picks it up. She extends her hand to seize the food. But her hand drops to the floor without touching it. She looks at the lettuce but (unless very hungry) does not touch it" (p. 75). Remember, as properties of molar behavior, these are *not* to be reduced to 'physiological motions,' nor, obviously, to electromagnetic vibrations, air vibrations, or compounds of chemical atoms. "Descriptively and per se they are other than those motions." They are synthetic qualities, genuine contextualistic qualities, expressing the nature of the events going on.

To clinch the point, let me call attention to the concept of 'fusion' so frequently employed by Tolman throughout his exposition. What could this term refer to except to qualities, and what would be the result of its action except a quality? His whole conception of the sign-gestalt depends on this concept of fusion, and the concept of the sign-gestalt is the keystone of his system. "What we mean by 'the Law of Fusibility,'" he says "is that certain characters of signs, means-end-relations and significates will undoubtedly fuse together into single sign-gestalt wholes more readily than will others. . . . Some characters will organize themselves into gestalt wholes more readily than will others. . . . This would mean, say, that certain types of to-be-conditioned stimuli, perhaps odors, should come to evoke salivary secretions, *i.e.*,

become a sign for coming food, more readily than others" (p. 380). Characters of odors, say, salivary secretions and food fused into a gestalt whole—that is exactly the sort of thing the contextualist means by the fused quality of a total texture. Quite apart, therefore, from the empirical evidence that Tolman is using synthetic qualities all the way through his exposition, we have the added proof of his implicit acceptance of these qualities in that his crucial concept of the sign-gestalt depends upon it.

When, therefore, Tolman writes that, "In place of the concrete, but ineffable, richness of real experience, as it comes, *i.e.* of our hopes, our feelings, our images, our thoughts, we have substituted a barren and 'unfelt' array of functionally defined immanent determinants and behavior-adjustments"—we can only look at these words in amazement. We did not find his immanent determinants and behavior adjustments 'barren and unfelt.' We found them on the contrary, an illumination of our hopes, our feelings, our images, and our thoughts. And so far from these latter being 'ineffable,' we imagined Tolman was spreading them out in their environment and describing them openly and objectively for us before our eyes.

In view of this conclusion, we cannot see the grounds for Tolman's implicit disparagement of introspection. His objection to introspection cannot be that it treats of qualities, for his own objective method treats of qualities. It cannot be that it confines itself to a conscious field insulated from the outside world, for Tolman, consistently with his method, has adequately defined conscious awareness "as consisting in the performance of 'sampling,' or 'running-back-and-forth' behavior" (p. 206), which is as integral with the world in general as any behavior. It cannot be that introspection is a mysterious or indescribable process, for Tolman describes it as a process in which "an 'observer' must be capable of sign-gestalts in which his own behavior-adjustments, and the words describing such, are the *sign-objects* (*i.e.* means-objects) and a presence-of-these-behavior-adjustments-to-the-listener is the desired *signified-object* (*i.e.* goal object)" (p. 241).

Tolman is even able to give a searching analysis of two kinds of introspection, 'Kundgabe' and 'Beschreibung,' and to estimate the informative reliability of each. His objection cannot be to introspection but only to the mechanistic (or, what is the same thing, the mentalistic) interpretation of introspection. Because his contextualistic method cannot accept the mechanistic interpretation of introspection, he seems to think that he must reject introspection altogether, and yet at the same time he offers an excellent and thoroughly workable contextualistic interpretation of introspection. There is nothing 'shameful' (p. 204) for a contextualist either in conscious awareness or in introspection. They do not even constitute a serious problem for a contextualist. They do, however, constitute a serious problem for the mechanist. The 'shame' Tolman alludes to is simply a hang-over from the violent suppression of this problem characteristic of the physiological behaviorist of the Watsonian type.

A study of Tolman's description of the behavior-act, therefore, worked out from his central factor of the immanent determinants, reveals the fact that he is a thoroughgoing contextualist. He accepts either implicitly or explicitly all the contextualistic categories. The two categories which he appears explicitly to reject (the textural character of every environment, and the qualitativeness of every texture) we find him implicitly employing all along; and employing in such a way that if his explicit rejection of these categories were accepted, his method would be violently disrupted. His method is, in truth, a *bouleversement complet* in respect to the mechanistic methods of the past.

V. The Contextualistic Nature of Initiating Causes, Behavior Adjustments and Capacities

It is to be noted, however, that up to this point we have been focussing our attention only upon the central factor of his system, the immanent determinants. What about the other factors: the initiating causes (the environmental causes, and the initiating physiological states), the purposive and cognitive capacities, and the behavior adjustments?

Concerning the behavior adjustments, I think nothing more needs to be said. As soon as Tolman accepts introspection into his system without any sense of 'shame,' these (the 'behavioristic substitute for what the mentalists would call conscious awareness and ideas') fit into the behavior act among the immanent determinants as neatly as do means-objects or any other recognized strands or sections of the act. Behavior adjustments are not a separate factor; they are simply an expression for a particularly complicated action of the immanent determinants. Tolman's elevation of these into a separate factor is but another instance of the hang-over of mechanistic modes of thought in his new system.

Nor do the initiating causes demand much comment. The setting of an act must be known in order that its course may be predicted. The environmental stimuli and the initiating physiological state are simply the outer and inner setting of the act. The only question is whether Tolman is not sometimes conceiving of both of these in too mechanistic a fashion. The environmental stimuli, of course, must be kept on the same molar level with the goal-objects and means-objects, with which they are, indeed, generally continuous. And the physiological states in the final description must be synthetic states and not too physiological. The initiating causes, in other words, must keep conceptually in mesh with the immanent determinants. In practice, Tolman always does see that this is the case, but in theory he seems here again occasionally to exhibit a hang-over from mechanistic modes of thought.

As regards the cognitive and purposive capacities, however, there is a great deal to be said—much too much. They lead one into a set of problems, the solution of which is by no means evident. Tolman has not clearly exhibited the status of these behavior capacities. Yet, since to my knowledge nobody has satisfactorily shown their proper status in a contextualism, Tolman is not open to serious criticism on this score. The problem, nevertheless, should be brought out and reflected upon.

Let me begin by quoting Tolman's definition of capacities

in the glossary: "Capacities are the endowments of the individual or the species which result from two of the initiating independent causes of behavior, viz., innate endowment and past training. An individual, or a species, responds to given stimuli and to given initiating physiological states, as it does, by virtue of the capacities which it possesses. Such capacities are to be subdivided, for the purposes of an initial analysis, into: discriminanda capacities; manipulanda capacities; means-end-capacities; retentivity; consciousness-ability and ideation ability; and creativity. This short list of capacities is to be conceived as having, however, but an initial tentative validity. It is to be supposed that ultimately, when the laws of heredity and of environmental effect are better known, the immediately given hereditary and training factors must be counted (at least in the case of man) as legion. The above list of capacities will then be considered not as the ultimate division of hereditary and training endowments but as a rough list of 'immediate response requirements' which must be satisfied by the hereditary and training factors if such and such responses are to be successful."

From this description, I think it is clear enough that Tolman himself is puzzled about the concept of capacities. They are obviously not exposed objective factors visible to all who look at a behavior-act as that act is going on. They lie outside the act so far as that is an actual behavior process. Tolman quite rightly segregates them from the immanent determinants. Capacities are not immanent in the act, in the sense in which a particular cognitive or purposive reference is immanent in the particular act. Yet (I think I am safe in saying this) every element of the act is immanent in that act only by virtue of its being an element of a capacity relevant to that act.

I was tempted here to say, "Every element of the act is immanent in that act only by virtue of its being an element of a capacity *of the organism* performing the act." But this would be a blurring of the situation, which we wish on the contrary to render more clear cut. For what is meant by

'of' in 'of the organism'? Apparently, 'of' means possession. But this kind of possession is not the literal sense of possession, as my possession of a house or of an automobile. A capacity is not a piece of economic property that can be bought and sold, lost or stolen, not in any literal sense what Tolman calls an 'endowment.' It is not in any usual sense of the term a physical, nor even an actual event or process. The term, as Tolman uses it, is probably a makeshift concealing a complicated relation. Can we get any light as to the nature of this relation?

Suppose we restrict our attention to one only of the kinds of capacities Tolman names, and see what specifically that capacity turns into and whence the evidence for it was derived. Let us take a discriminanda capacity. This is a good choice, for Tolman seems to regard the descriptions of discriminanda capacities as models for the future descriptions of other kinds of capacities not yet fully worked out.

A discriminandum is a specific sensory expectation (or support), such for instance as a shade of red employed as an object of response. The discriminanda capacity of any organism for color is the scheme of all the colors to which the organism is capable of responding. This varies from species to species and even for members (for instance, color-blind) within a single species. The determination of this specific scheme requires a reference to a standard scheme, the color pyramid. It may, however, be necessary to supplement this standard scheme (which is based only on human discriminations) with more abstract materials such as light wavelengths, in order to deal with organisms having finer or more extensive discriminations than ours.

The important point to note is that, in the first instance at least, a discriminanda capacity for color is a scheme. We must digest this fact fully before we go further. So far as a discriminanda capacity is employed by a psychologist in the description or prediction of an act of behavior, a scheme is all that it resolves itself into. An organism's response to a specific color is psychologically described in its specificity by reference to a certain volume in the color pyramid. That scheme is the psychologists' tool for accurate description and

control, and as such a tool it is rightly regarded by the psychologist as a factor in the *description* of the behavior act. But is it a factor in the act itself?

The scheme as a whole is not a factor in the act itself, but only that element of the scheme which functions in the act. The color pyramid is not a factor in an act requiring a color discrimination, but an element in the color pyramid in some sense is. The relation between the color pyramid and the act is that of a reference of a factor of the act to an element of the color pyramid.

But is there not more involved in a capacity than that? Does not a capacity abide in an organism? In a certain sense it does, and that sense is what we call the structure of the organism. But if capacity were limited to this sense, it would for any single act be equivalent to the physiological state of the organism at the time of that act. And physiological state, as a factor in the act, has already been accounted for.

A capacity, therefore, is not just a scheme such as the color pyramid, because such a scheme floating at large has no obvious relevancy to an act. Nor is a capacity just the physiological state of an organism (or any similar factor in the act) because such a state is an actual structural condition of an organism and a mere fact. A capacity is, it would appear, a relation between a physiological state and a scheme. The physiological state of an act indicates the scheme relevant to that act, and the scheme indicates the alternatives open to that act by virtue of the physiological state to which the scheme is relevant. In other words, the scheme describes the potentialities of the relevant physiological state, and the physiological state guarantees the relevancy of those potentialities to the act in which the physiological state is a factor. That seems to be what a capacity is.

Now, if a capacity is such a relationship of a scheme to a factor of an act, and if, as seems to be the case, the scientist's predictive powers lie in his schemes, we should naturally expect for the full control of the act that the scientist would seek to develop schemes for every discoverable factor of his act. Every factor of an act would then have a scheme

parallel to it, and the relation between the two would be a capacity. There would thus be as many capacities as there are factors into which an act is analyzable. The more complex the act, the greater the number of capacities involved.

These capacities, however, should all be conceived of in molar terms. For the capacities must be in the same sort of terms as the factors of the act. Tryon's point, therefore, to the effect that there must be a very large number of factors in order that the distribution curves should be smooth, seems to me not to follow. If a factor of a behavior act is, by definition, a molar unit, it may very well be that this unit is itself the result of a multiplicity of molecular units. The molecular unit differently distributed in different individuals would give a smooth distribution curve for the molar unit. The molar unit would nonetheless be a single factor of behavior. And in contextualism it is the larger unit that determines the nature of the act rather than the smaller. Tryon is thinking in mechanistic terms, and for the moment Tolman gives way to Tryon's reasoning. For Tryon is assuming that the *real* factor is the most elementary that can be reached. But contextualistically the *real* factor is the most synthetic that can be utilized. There are *really* probably not many behavior capacities, but each behavior capacity is complex in nature.

VI. Summary of the Descriptive Uses of Tolman's Concepts

Let me now briefly summarize this analysis of the conceptual instruments Tolman has developed for the description of acts of behavior. His descriptive concepts fall into two groups. One group is an articulated scheme for the patterning of any particular completed act. The other group is a set of schemes predicting, on the basis of information gathered outside the particular act, what the organism can do under specific conditions. The first group comprises what may be called the patterning concepts, the second the capacities.

The main concepts of the first group are environmental stimuli, initiating physiological states, purposive references, cognitive references, means objects, goal objects, sign-ge-

stalts. Such concepts as drives, appetites, and the like, are derivative in various ways from the concepts just listed. By means of these main concepts, integrated in various patterns, any single act is mapped out.

But the specific filling for these patterning concepts comes from the capacities. The capacities furnish the materials available for the various factors of the act. Thus the discriminanda capacities indicate the range of sensory discrimination open to a given organism, the manipulanda capacities the range of muscular skills open to the organism, means-end capacities the range of instrumental references open to the organism, and so on. Specific items from these various ranges or capacities are selected and set in the pattern of the patterning concepts to produce the full description of any individual concrete act.

The patterning concepts have the function of laying out the specific plan of each individual act. The capacities have the function of supplying the names and the quantitative determination of the elements going into each part of the plan. Between the two, any act is completely described. Moreover, given the capacities and part of the plan of an act, and the rest of the act can with considerable certainty be predicted.

In conclusion, it is only fair to remark, what must be sufficiently obvious, that Tolman should not be held responsible for all the details and implications of the foregoing analysis. I seem to perceive in his concepts and his method a revolution of the first order in the field of psychology, a revolution pregnant with possibilities and one that will do away with the difficulties connected with the traditional mechanistic (and 'mentalistic') psychologies of the past. It is, moreover, a movement in the same direction as one now going on in physics, where an operationalism seems to be taking the place of the nineteenth century mechanism as the official philosophy of science. But I may, nonetheless, in the interests of consistency have drawn Tolman's concepts into implications which he may never wish to concede.

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THE CONCEPT OF THE HABIT-FAMILY HIERARCHY AND MAZE LEARNING: PART II

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VI. Automatic Habit Transfer when the Principle of Frequency must be Overridden

We must now consider the question of how the tendency to take the long path, which is clearly dominant with many of the animals in Valentine's experiment at the beginning of the 'critical runs,' *i.e.*, that portion of the practice in which the door at *E* was open throughout each trial, finally gives place to a dominance of the tendency to take the short path. We have shown in the preceding section how occasional choices of the shorter path might be expected to take place through the influence of chance sensory disturbances. But in many cases, at least, the external inhibitions at *E* would be of relatively infrequent occurrence and the proportion of long-path to short-path choices would still greatly favor the choice of the longer path. Suppose, for example, that we assume only one short choice out of six. A naïve interpretation of the principle of frequency would demand that such a ratio not only would not increase the proportion of short-path choices but that the short-path choices should steadily decrease until they would entirely disappear. Such an interpretation of the principle of frequency implicitly assumes that frequency is the only principle operative. It is to be doubted whether anyone has ever utilized this principle under such an assumption. At all events, there is reason in this particular case to believe that at least two other factors are also operating simultaneously and in such a way as to oppose and override it.[14]

[14] To over-ride a principle or factor by opposing to it one or more factors which possess in the aggregate a greater strength should not be considered as a disproof of the existence and significance of the first principle. Thus, for a balloon or an airplane or a thrown object to rise from the earth in opposition to gravity is in no sense a proof of the non-existence of gravity or that gravity need not be taken into consideration, as many an aviator has found to his sorrow.

The first opposing factor to be adduced in this connection is the empirical principle discovered in conditioned reaction experiments that the restoration of an experimentally extinguished tendency to action is much more rapid than was its original acquisition. Thus Hilgard and Marquis (9) report that the restoration of an experimentally extinguished lid reaction recovered in the case of three dogs in approximately one-fifth, one-fourth, and two-fifths respectively of the numbers of reinforcements originally required to produce the same increase in strength of the tendency. If we may take the middle of these three values as a first rough approximation and apply it tentatively to Valentine's rats, this principle alone would allow the animals to take the long path three times out of four and still have a substantial margin of practice effects in favor of the shorter path.

A second principle which presumably contributes to the same end is that of the goal gradient (16). Let us assume in the interest of simplicity of exposition that increase in strength of excitatory tendency per repetition within a moderate range is constant under given experimental conditions. Now, from the choice point *E* in Valentine's maze, the long path is nearly four times as great as the short one. Turning to the table of hypothetical goal gradient values (16, 32) and giving the short and the long paths distance values of 10 and 38 respectively, it is easy to see that one repetition of the short path will strengthen the tendency to repeat that act on a subsequent occasion much more at point *E* than one repetition of the long path will strengthen the latter tendency at the same point. Under these assumptions the short path would increase in strength per trial in the proportion of 4.28 whereas the long path would increase in strength per trial in the proportion of only .96. Thus, by this principle alone the short path would ultimately become dominant, even though it were chosen only a fourth as often as the longer one.[15] This pro-

[15] Suppose that a given degree of perfection would be attained in ten repetitions. By 16, p. 32, each trial would increase the strength of the tendency to take the short path by .428 unit, whereas it would increase the strength of the tendency to take the long path by .096 unit. It follows that two runs on the short path would strengthen that tendency by .856, whereas eight runs on the long path would increase the strength of that tendency by only 8 × .096, or .768 unit. This would leave a net advantage of .088 to the tendency to take the short path.

portion would vary with the absolute size of the maze, but the general principle ought to hold if the goal gradient hypothesis is sound.

We do not yet know enough about the two processes just considered to be able to say whether they would combine in an additive or a multiplicative manner, but it is evident that they would summate in some way. The nature of the relationship would seem to favor a multiplicative mode of combination. If that were true, the above numerical assumptions would lead to the expectation that an ultimate giving up of the blind alley in Valentine's experiment during the 'critical' runs would still take place even if disinhibition should occur on the average in only one trial out of ten or twelve. By similar reasoning, the more frequently disinhibition takes place at point *E*, in this stage of the learning process, the more rapidly the blind will be eliminated. On the other hand it is conceivable that distractions might be so far eliminated that occasional animals would never make the correction at all. From this general angle may possibly be explained the stubborn cases of 'fixation' which have puzzled many experimenters; it may be that certain animals, by constitution or the accidents of life, are relatively immune to disinhibition.

VII. The Predilection of Animals for Entering Goalward-Pointing Blinds

We come now to the consideration of the behavior of the rat in an ordinary single-path maze such as that shown in Fig. 13. Experimentalists frequently report two characteristic phases in the process of a hungry rat learning a maze. The first is a slow, hesitant groping until the food is found. Soon after, the behavior of the rat changes markedly: he goes about his searching in a business-like way, as if he were 'going somewhere' (6). The same thing is often expressed by saying that the rat has become oriented. It will be recalled that according to the hypothesis here being presented, orientation itself consists in the anticipatory goal reaction (r_G) becoming conditioned to the external stimulus complex S_1' (Figs. 10 and 11). But we have already seen that when

this takes place the practice effect of traversing the path by which the food (*F*) was reached is at once transferred to all other members of the particular habit family, especially to those which are shorter and therefore occupy a superior position in the hierarchy. It follows, provided the animal

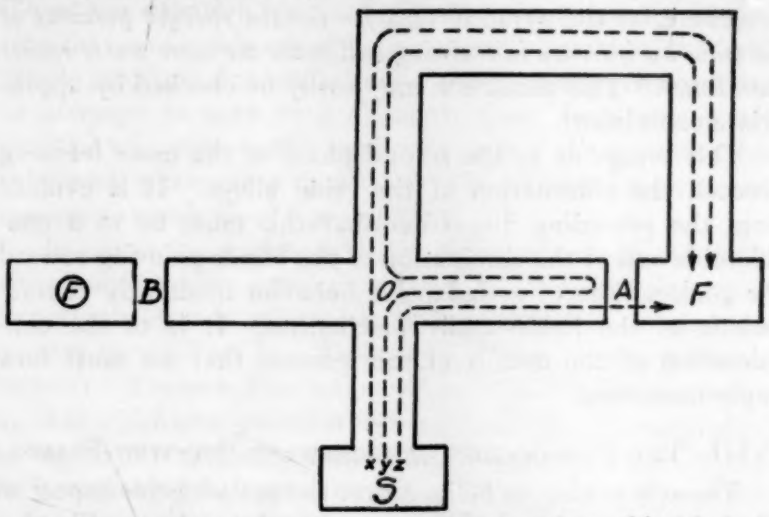

FIG. 13. Conventional maze to represent the predilection of animals for entering blind alleys which point in the direction of the place where food is obtained. The *F* in the circle at the left of *B* represents food inaccessible to the animal but placed in such a position as to give the same olfactory cues at the choice point *O* as that which is ultimately eaten. The broken lines indicate three conventional paths; path *Z* represents the path of the preferred member of the habit-family hierarchy which the animal would take if the blind end of alley *A* did not prevent. It is to be noted that this maze would not be suitable to test experimentally the existence of the tendency to enter goal-pointing blinds because it would also evoke the tendency to make an anticipatory right turn (22, 30).

cannot see the end of the alley in question, that every blind is the beginning of a path to the goal. It is obvious that blinds like *A*, Fig. 13, pointing in the direction of the goal, are the beginnings of paths which, if completed, would be among the shorter ones of the animal's habit family and hence are to be preferred to the longer, more roundabout path, *X*. It accordingly follows at once that *animals will show a marked predilection for blinds pointing in the direction of the goal, as contrasted with alleys pointing in the opposite direction.*[16]

[16] Both Dashiell (1) and Tolman and Honzik (33, 119 *ff.*) have reported this significant phenomenon on the basis of experiment.

As a corollary of the foregoing, it follows directly that for blind alleys pointing in the direction of the food box there will be a period in the learning of the maze, *i.e.*, through the acquisition of orientation, during which these errors will progressively increase. Thus we arrive, by a purely deductive procedure, at the paradox that *for certain specific portions of the maze an increase in training will make the score worse rather than better.* This deduction may easily be checked by appropriate experiment.

This brings us to the second phase of the maze learning process, the elimination of the blind alleys. It is evident from the preceding discussion that this must be to a considerable extent the elimination of the blinds pointing toward the goal, a form of maladaptive behavior inherently characteristic of the habit-family mechanism. It is to the consideration of the details of this process that we must now apply ourselves.

VIII. The Elimination of Goalward-Pointing Blinds

There is reason to believe that the maladaptive aspect of the habit hierarchy system just referred to, the predilection for entering blinds pointing in the direction of the food box, is corrected by at least two tendencies. Each of these is fairly distinct, and both presumably operate simultaneously. The first and more basic mechanism is that of the direct effects of the goal gradient (16) which would make path *X* (Fig. 13) preferable to path *Y* because the former is shorter. This principle, however, cannot alone account for the final preference of path *X*, unless there is assumed to be a moderate number of spontaneous choices of the shorter of the two paths during the early practice trials.[17] But the habit-family

[17] On the principle of the goal gradient alone, assuming the latter to operate according to the logarithmic law, and, in the interest of simplicity of exposition, assuming an approximately straight curve of learning, it would be possible for the choice finally to settle on path *X*, even though there should be distinctly more trials on *Y* than on *X*. Path *X* from point *O* is about 27 units long, and path *Y*, about 41. By Table 2 of (16), path *X* should have, at point *O*, an excitatory strength in the proportion of 1.81, whereas path *Y* would have a strength in the proportion of .77. Since .77 is contained in 1.81 twice with .27 left over, it follows that path *X* would finally come to preponderate over *Y* even if the former had initially only half as many choices as the latter.

principle, as applied to paths *X* and *Y*, would, by virtue of the principle of transfer, shift the dominance of reaction from *Y* to *Z*, once orientation had taken place on the basis of runs in *Y*, *even though not a single run had ever been made by path Z.* Thus, the situation is complicated by the necessary implication that, as already noted in the preceding section, the habit-family principle also produces the strong maladaptive tendency to attempt to take path *Z*, quite apart from any specific practice whatever on this particular path. We must now consider what phenomena should be expected under these circumstances to result from repeated trials.

In order to understand the outcome of this complex situation, it will be necessary to put forward a supplementary hypothesis already referred to and employed in another connection (p. 49, Part I). This may be called the *frustration hypothesis.* The term frustration is here used to indicate any situation in which an acquired excitatory tendency, and particularly an excitatory tendency located posterior to a chain of such excitatory tendencies making up an appreciable segment of a behavior cycle, is for any reason prevented from evoking its accustomed reaction. The hypothesis is that *under such circumstances internal inhibitions will be developed which will manifest substantially the same characteristics as result from the experimental extinction of conditioned reactions.* Among the phenomena to be expected on this hypothesis are the gradual cessation of attempts to perform the act which has been frustrated, spontaneous recovery of the tendency with the passage of time, and disinhibition (**25**, 66).

It will be necessary also to recall in this connection the famous study of Ebbinghaus (**4**; **7**). In that investigation he obtained experimental evidence indicating that in series of nonsense syllables learned by rote, there were set up by the learning process excitatory tendencies not only connecting each syllable with the next but also remote excitatory tendencies between syllables as far apart as seven intervals in the series (**4**, 106). It is the existence of these remote excitatory tendencies that is of special significance in the present connection.

With these principles especially in mind, let us proceed to the consideration of what might logically be expected to take place in the behavior of the rat upon repeated entries into blind alley *A* of Fig. 13. Suppose that the stimulus and behavior flux of potential path *Z*, from choice point *O* to the goal *F*, be represented by the arbitrary action phases shown in Fig. 14, with the barrier (the end of the blind) falling just

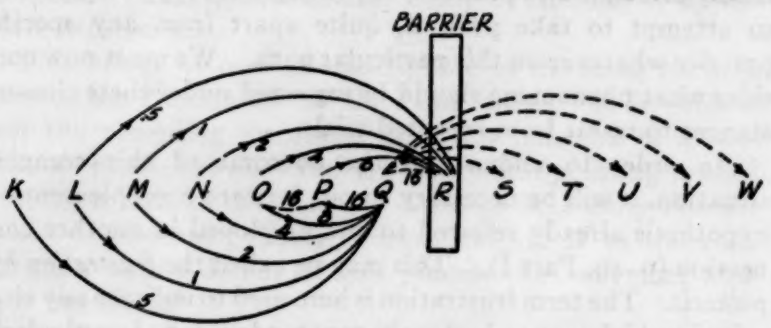

FIG. 14. Diagrammatic representation of some of the immediate and remote excitatory tendencies involved in a typical behavior sequence where an animal in a maze enters a blind alley pointing in the direction of the food box. The letters *K*, *L*, *M*, *N*, etc., represent both stimuli and reactions at closely spaced intervals in the behavior flux. For purposes of simplifying the exposition, the goal gradient as such is ignored. The gradient of the remote excitatory tendencies of Ebbinghaus is represented conventionally by giving each value one-half the strength of that preceding it. Each point is understood as having six excitatory tendencies emanating from it when considered as a stimulus, and six excitatory tendencies converging upon it when considered as a reaction. Only a few of these could be represented without unduly complicating the figure.

before reaction *R*. Now, according to the Ebbinghaus hypothesis, there should be an immediate excitatory tendency with a strength of 16 units, say, from *Q* to *R*; a remote excitatory tendency of less strength, say one-half as great (8 units), from *P* to *R*; a second remote excitatory tendency, one-fourth as great (4 units), from *O* to *R*, and so on. The excitatory tendencies of this series will yield a total aggregate excitatory tendency of approximately 32 units. It is important to note that because of the habit-family principle there will be at all times, in addition to the tendency of path *Z* to lead into alley *A*, a relatively weaker excitatory tendency to go *out* of alley *A* by path *Y*. Let us assume that this latter and opposing tendency has a strength of 14 units.

But the barrier, by preventing act *R* from taking place, according to the frustration hypothesis, will produce an inhibition at all points from *L* to *Q*, presumably of a strength at each point in proportion to the strength of the original excitatory tendency from each point to *R* (**25**, 100). Now it is known that when one excitatory tendency suffers extinction, the resulting internal inhibition has a tendency to irradiate, as secondary inhibition, to closely related excitatory tendencies (**25**, 61, 171). Thus it would be reasonable to suppose that the frustration of the 8-unit remote excitatory tendency from *P* to *R*, together with probable additional inhibitions resulting from the frustration of the excitatory tendency from *P* to *S*, *P* to *T*, *P* to *U*, etc., will spread in such a way as to inhibit to a considerable extent the immediate excitatory tendency from *P* to *Q*, and to a progressively lesser extent to the excitatory tendency from *O* to *P*, from *N* to *O*, from *M* to *N*, and so on. Thus there would result from the frustration at *R* *an inhibitory gradient extending backward from R as a focus* at which point it presumably will be nearly equal to the inhibitory tendency upon which it is based. This means that there must be a parallel but reversed excitatory gradient extending backward from *R*, at which point the effective limiting excitatory value would be zero.

Now, by hypothesis, the moment that inhibition has weakened the excitatory tendency to go forward at any point below the assumed level of 14 units, the animal will turn and take path *Y* out of the blind to the food box *F*. It is clear that this turning must take place first at point *R*, since that is the initial focus of frustration at which the inhibition is maximal. Upon repetitions of frustration at successive trials, the inhibition (and so the secondary inhibition) will grow more profound, the gradient at the same time extending farther backward beyond *M*, *L*, etc. As a result, the critical excitatory level of 14 units on path *X* will move backward toward *L* progressively and as this takes place the point at which turning occurs will also move backward. But when the turning point begins to move backward, *e.g.*, when the turning takes place at *Q*, this itself will create a

new set of immediate and remote frustrations substantially like those which resulted when the barrier prevented act *R* from taking place. This new set of inhibitions will cause the animal to turn and follow path *Y* at a still earlier point; the earlier turning will produce another new crop of frustrations, which will result in a still earlier turning, and so on. *Thus the animal may be expected, in general, to penetrate the blind alley shorter and shorter distances on successive trials until finally he will not enter it at all*, but will proceed directly to the food by way of path *X* (3; 17).[18]

It should be noted that this explanation of the elimination of a blind alley is distinctly related to the important hypothesis put forward by Holmes around 1910 (11, 164 *ff.*). Holmes' principles, however, are inadequate to explain the elimination of a blind alley from a maze because they do not show how the end of an alley can lead to an 'incongruous' act (10). The checking of locomotion or the necessity of turning about at the end of a blind alley only becomes 'incongruous' on the assumption that there is a definite excitatory tendency to go directly through the space occupied by the end of the blind. This is not provided by Holmes' hypothesis. There is no reason to believe, for example, that a rat would develop any inhibition from going into a blind alley during random exploration, for the reason that in such a case there would exist to be frustrated no excitatory tendencies to go anywhere in particular. The principle of the habit-family hierarchy furnishes a clear basis for a definite frustration, and the results from conditioned reflex experiments furnish a principle by which this frustration could be converted into an inhibition without which repetition could hardly produce the elimination. It also affords a deduction of the *mode* of elimination, *i.e.*, the progressively shortened entrances, a matter of considerable corroborative significance.

[18] Several writers, perhaps the earliest of whom is Peterson (26), have noted the progressively shortening penetrations of animals into blinds. Perhaps the most convincing study illustrating this tendency is that by Robinson and Wever (27), a truly beautiful experiment. Though the conditions of this latter investigation differ in certain respects from those assumed in the above deduction, it is believed that the explanation here offered holds substantially for them also.

IX. Eight Corollaries involving Blind Alley Elimination

A number of corollaries flow from the deduction presented in the preceding section:

The first concerns the relative ease of eliminating long vs. short blinds. It has been shown elsewhere (16) that the mechanism of the goal gradient alone would lead to the more ready elimination of long than of short blinds. Since, other things equal, alleys not pointing toward the food box are believed to be eliminated largely by the goal gradient mechanism, it follows that with such alleys hungry animals will eliminate long alleys more readily than short ones.

In the case of alleys pointing in the direction of the goal, however, the principles put forward above lead to a different expectation. Since the elimination is a progressive or stepwise process, it would seem that the deeper the alley, the more steps of a given length it would require to complete the elimination. Consequently long alleys pointing in the direction of the food box should be eliminated less readily than short ones.

The third corollary concerns the behavior of the animal between *S* and *O*, Fig. 13, when he is about to discontinue entering *A*. Because of the progressive backward movement of the inhibitory gradient, the forward excitatory tendency of path *Z* should show a perceptible degree of weakening, presumably in the form of reduced speed, for some distance preceding the arrival at the point at which the turning takes place. It would seem logical to expect that as the critical turning point reaches the entrance to the blind (*O*) this antecedent segment of inhibition should have spread downward on path *Z* towards *S*. Since inhibitions are known to persist to a certain extent, this should manifest itself by a reduction in the speed of locomotion as the animal approaches *O*, even on occasions when he has ceased to show any other tendency whatever to enter alley *A*.

A fourth corollary is dependent on the additional assumption that the dominant principle of the elimination of alleys pointing away from the goal depends upon a rather different

principle, *i.e.*, upon the competition at point *O* (16, 34) between the excitatory tendencies to take paths of the *X* and *Y* type. If the *Y* path should triumph on any given occasion, it is difficult to see what, save some scant and rapidly fading inhibitions resulting from the conflict, could prevent the animal from running the whole length of the alley at each entrance, or at least to a point from which the blind end would be visible (17). But since alleys pointing away from the food box are the beginnings of possible, even though non-preferred, paths to the goal, there probably would be some tendency to inhibitory phenomena here also, though it should be weaker than in alleys pointing toward the food box. Accordingly it is believed that if an average were made of the depth of penetration into the two types of alleys on those occasions where a clear tendency to enter appeared, the alleys pointing away from the goal would display the greater mean depth of penetration.

A fifth corollary is that, since the failure to enter alley *A* is essentially an inhibitory phenomenon, some disturbance like the weak sounding of a buzzer taking place when the animal is on his way between *S* and *O* should produce a disinhibition; in which case the animal might be expected again to enter alley *A*. If a much stronger disturbance should take place it ought also to produce external inhibition of the movements constituting path *X*, in which case the animal would be likely to cease locomotion altogether. Moreover, upon resumption of locomotion the animal might be expected to go sometimes into alley *B*, though there would still probably be somewhat of a predilection for entering alley *A*.

But since the results of both of these types of disturbance are distinctly transitory (25), it may be anticipated with some confidence that after a few minutes of quiet following entrances into the blinds the animal's behavior should show little or no tendency to deviations from its former errorless performance.

The fact of spontaneous recovery from experimental extinction demonstrates that inhibitory tendencies disintegrate more rapidly with the elapse of time than do the

excitatory tendencies upon which they are based. It seems likely that the principle of the more rapid disintegration of inhibitory tendencies may be rather general, regardless of their origin (**25**, 66). If we are warranted in making this assumption, it follows that the frustrational inhibitions by which goalward-pointing alleys are eliminated would accumulate more slowly if a few hours should intervene between repetitions than if they are *immediately* consecutive, *i.e.*, other things equal, goalward-pointing alleys will be eliminated less easily by distributed than by massed practice. This principle would be much less active in alleys pointing away from the goal because their elimination is less dependent on inhibition. It follows that with distributed practice there should be relatively a greater difference between the ease of eliminating the two types of alleys than with massed practice, particularly where the alleys appear near the posterior end of the maze.[19]

It is stated (**5**, 365) that caffeine tends to facilitate the acquisition of excitatory tendencies, but to retard the development of inhibitions. In this connection it will be remembered that alleys pointing away from the goal are believed to be eliminated largely on the basis of competing excitatory tendencies, whereas the major factor in the elimination of blinds pointing toward the goal is believed to be inhibitory in nature. If we make the rather dubious assumption that the action of caffeine on the nervous system is as simple as just stated, the above considerations lead to a further corollary: rats learning a maze under the influence of caffeine will eliminate alleys pointing away from the goal more readily

[19] It is easy to deduce from Lepley's hypothesis (**21**) that rote series of nonsense syllables will be more easily learned by distributed practice. Presumably the same principles apply to mazes, particularly if most of the alleys point away from the goal. This would explain the well-known economy in maze learning by distributed practice. The action of the Lepley principle and the present one are thus in opposite directions, with the evidence favoring the dominance of the former. Even so, the above hypothesis seems to demand that there should be relatively less advantage from distributed practice on mazes made up largely of goalward-pointing blinds than on mazes made up largely of blinds pointing away from the goal. At the very least the predominant direction of the blinds becomes a matter of significance in maze studies involving distributed vs. massed practice.

than when in the normal condition, but will eliminate alleys pointing toward the goal with greater difficulty than when in the normal condition. Related implications may also be drawn from the alleged action of bromides (5, 365).

X. The Concept of Habit-Family Hierarchies based on Subordinate Goals

The above deductions from the concept of the habit-family hierarchy are drawn, for the most part, from the relatively simple situation in which the goal reaction (r_G) or some closely related reaction is the dominant factor. There is evidence, however (30; 22; 23), that distinctly subordinate goal reactions also tend to come forward in behavior sequences in a manner substantially similar to goal reactions themselves, though doubtless less strongly. A large number of experimental phenomena may be deduced, and thus explained, on this assumption by methods analogous to those employed above. Space here is sufficient only to list two or three major type situations.

The first is dependent upon positive transfer on the basis of an r_G from a subordinate goal reaction. Here belong Maier's ingenious experiments which indicate that even in the rat the more remote end of a segment of a pathway leading to the goal is able to attract to itself, though weakly, a second segment appropriate in combination to make it possible for the animal to reach the food box (24). The second type is represented concretely by a nice experiment reported by Tolman and Honzik (35; 34). In this study, evidence was obtained which indicates that when an animal encounters a barrier in the segment of a path common to the terminal portion of two alternative paths leading to food, there results an inhibition not only of a previously existent tendency to enter the path traversed on the occasion of the frustration, but of the tendency to enter the other path as well. This phenomenon is explained on the basis of negative transfer, *i.e.*, the transfer to one path of an inhibition acquired in association with the other.

But once the principle of transfer on the basis of sub-

ordinate habit families based upon the anticipatory nature of subgoals is recognized as a general proposition, each subordinate goal with its habit family may be conceived as possessing numerous goals of a still lower order, each with a lower-order habit-family hierarchy, and so on. In this way the principle of transfer obviously may be extended downward almost indefinitely to even the most minute details of behavior adjustment. On this hypothesis the rich variability, the almost fluid flexibility of behavior characteristic of mammals even in well-practiced action sequences, is a phenomenon not only wholly to be expected from the present point of view, but definitely demanded by it.

XI. Summary

We may now summarize our conclusions in brief dogmatic form.

The convergent excitatory mechanism is capable of mediating the transfer of reactions from one stimulus situation to another where the two stimulus situations externally may contain no common element whatever. When the divergent and the convergent mechanisms are combined, there emerges a new mechanism which may be called a habit-family hierarchy. The hypothesis is put forward that when an organism for any reason succeeds in solving a problem by a sequence of acts which is substantially that of one member of a habit family, the other members of the family, particularly those occupying a more favored position in the hierarchy, will automatically become active in the new situation without any specific practice whatever. This transfer may be thought of as being mediated mainly through the anticipatory reaction (r_G) which is common to all members of the habit-family hierarchy and which therefore as a stimulus is conditioned to evoke the initial reactions of each member. It seems likely that this mechanism of transfer may prove to have an extremely wide application as an explanatory principle in many subtle and otherwise inexplicable forms of behavior at present usually designated indiscriminately as intelligence. The habit-family hierarchy is accordingly put forward as one of

the basic mechanism of insight; presumably there are numerous other such mechanisms. Thus we seem to be on our way to an objective and discriminating definition of this immensely important but badly understood phenomenon.

In making the application of the principle to the particular problems of maze learning in the rat, it is assumed that a great number of habit families of the locomotor variety have been established by the locomotion of the animal in free space during its early life. From these principles a number of deductions may be made, though in some cases the certainty of the deductions is limited by uncertainty as to the facts concerning conditioned reflexes upon which they are based. The more important of these deductions are as follow:

1. Animals which through lack of practice or otherwise have not had a normal opportunity to set up initial habit-family hierarchies, should show little or no signs of transfer.

2. Other things equal, immediate transfer in the maze to a shorter path should be distinctly more effective where the drive stimulus in the old and the new situations is the same than where it is different.

3. In case an animal is specifically trained to go into a blind alley, he will, if the training has not been too extensive, eliminate the blind at the first opportunity to take the direct path.

4. If the training to take the blind has been very extensive, the animal will normally continue to take the blind if undisturbed, even after the door which would permit him to take the direct path is opened.

5. If, under these conditions, the animal is disturbed shortly before the choice point is reached, he will be likely to omit the blind and take the direct path.

6. Shortly after such a choice, provided the disturbing stimulus has ceased, the animal will be likely to revert to his accustomed use of the blind, though in general the tendency to take it will not be so strong as it was previously.

7. If such disturbing stimuli should repeatedly produce the choice of the shorter path, even though interspersed with a considerably larger number of choices involving entrances into

the blind, the animal will come ultimately to choose the shorter path on most occasions.

8. The more frequently, relatively, choices are made as a result of disturbing stimulations, the more quickly will the animals acquire a fixed habit of choosing the direct path.

9. Animals in learning a maze will tend to show a predilection for blinds pointing in the direction of the food box, as contrasted with blinds originating near the same point and leading in the opposite direction.

10. Of two blind alleys pointing in the direction of the food box, other things equal, the one closer to it by direct line will be more favored by the animals and will be more difficult of elimination from the path.

11. Blinds pointing in the direction of the food box will show at first an increase in the percentage of entrances per trial, after which there will follow a gradual decrease. Blinds pointing away from the goal, other things equal, will show in general a progressive decrease in per cent of entrances from the beginning of practice.

12. On successive complete entrances to a blind pointing in the direction of the goal, the animal will proceed at a progressively slower pace especially as he approaches the blind end, whether the latter is visible or not.

13. The process of eliminating blinds pointing in the direction of the goal will be characterized by the animal turning around and coming out before reaching the end, and even before being able to see the blind end.

14. The depth of such partial entrances will tend to become less and less with continued practice, until finally the blind will not be entered at all.

15. The mean depth of all penetrations after the first partial entrance has taken place will average less for alleys pointing in the direction of the goal than for those pointing in the opposite direction.

16. Short blind alleys pointing away from the goal will be less readily eliminated by hungry animals than will long blinds.

17. Short blind alleys pointing in the direction of the goal

will be more readily eliminated by hungry animals than will long blinds.

18. After the animal has consistently eliminated a blind alley and has made repeated perfect runs, a slight disturbance just before he reaches the opening to such a blind will be likely to cause an entrance into the alley. A slightly stronger stimulus should cause an entrance also into alleys extending in the opposite direction. Such tendencies to enter blinds, however, should be of relatively short duration.

19. With distributed practice there should be a relatively greater difference between the ease of eliminating goalward and nongoalward-pointing blinds than there would be under immediately consecutive massed practice.

20. Alleys pointing in the direction of the food box should be eliminated with greater difficulty when the animal is under the influence of caffeine, whereas alleys pointing away should be eliminated with greater ease.

Several of the above deductions are known on the basis of experiment to be true, though many of them have never been subjected to experiment. The latter may, therefore, present an opportunity to test the truth of the basic hypothesis from which they logically flow. Hypotheses (in psychology too often called theories) are not matters of faith to be defended with religious zeal. On the contrary they are to be subjected to the most rigorous experimental tests possible at the earliest feasible opportunity. Those hypotheses which clearly fail by this test must be ruthlessly discarded. And, by the same token, no hypothesis which fails to yield such deductions (as distinguished from the intuitions of its originator or protagonists) has any status in science.

REFERENCES

1. Dashiell, J. F., Direction orientation in maze running by the white rat, *Comp. Psychol. Monog.*, 1930, 7, No. 32.
2. De Camp, J. E., Relative distance as a factor in the white rat's selection of a path, *Psychobiol.*, 1930, 2, 245–253.
3. Dennis, W., A study of learning in the white rat, *J. Genet. Psychol.*, 1930, 37, 294–308.
4. Ebbinghaus, H., Memory (Trans. by Ruger), New York: Teachers Coll., Columbia Univ., 1913 (original date, 1885).

5. Evans, C. L., Recent advances in physiology, 4th ed., Philadelphia: P. Blakiston's Son and Co., 1930.
6. Gengerelli, J. A., Preliminary experiments on the causal factors in animal learning, *J. Comp. Psychol.*, 1928, 8, 435–457.
7. Hall, M. E., Remote associative tendencies in serial learning, *J. Exper. Psychol.*, 1928, 11, 65–76.
8. Higginson, G. D., Visual discrimination in the white rat, *J. Exper. Psychol.*, 1926, 9, 337–347.
9. Hilgard, E. R., and Marquis, D. G., Acquisition, extinction, and retention of conditioned lid responses in dogs (to appear).
10. Hobhouse, L. T., Mind in evolution, London: Macmillan and Co. Ltd., 1926.
11. Holmes, S. J., The evolution of animal intelligence, New York: Henry Holt and Co., 1911.
12. Hull, C. L., Quantitative aspects of the evolution of concepts, *Psychol. Monog.*, 1920, 28, No. 123.
13. ——, Simple trial-and-error learning: a study in psychological theory, Psychol. Rev., 1930, 37, 241–256.
14. ——, Knowledge and purpose as habit mechanisms, Psychol. Rev., 1930, 37, 511–525.
15. ——, Goal attraction and directing ideas conceived as habit phenomena, Psychol. Rev., 1931, 38, 487–506.
16. ——, The goal gradient hypothesis and maze learning, Psychol. Rev., 1932, 39, 25–43.
17. ——, Differential habituation to internal stimuli in the albino rat, *J. Comp. Psychol.*, 1933, 16, 255–273.
18. Johnson, H. M., Audition and habit formation in the dog, *Behav. Monog.*, 1913–15, 2, No. 8.
19. Koffka, K., The growth of the mind (Trans. by Ogden), New York: Harcourt, Brace and Co. Inc.; London: Kegan, Paul, Trench, Trubner and Co. Ltd., 1925.
20. Lashley, K. S., The effects of strychnine and caffeine upon the rate of learning, *Psychobiol.*, 1917, 1, 141–170.
21. Lepley, W. M., A theory of serial learning and forgetting based upon conditioned reflex principles, Psychol. Rev., 1932, 39, 279–288.
22. Lumley, F. H., An investigation of the responses made in learning a multiple choice maze, *Psychol. Monog.*, 1931, 42, No. 189.
23. ——, Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 1932, 15, 331–342.
24. Maier, N. R. F., Reasoning in white rats, *Comp. Psychol. Monog.*, 1929, 6, No. 29.
25. Pavlov, I. P., Conditioned reflexes, an investigation of the physiological activity of the cerebral cortex (Trans. by Anrep), London: Oxford Univ. Press, 1927.
26. Peterson, J., The effect of length of blind alleys on maze learning: an experiment on twenty-four white rats, *Behav. Monog.*, 1916–19, 3, No. 15.
27. Robinson, E. W., and Wever, E. G., Visual distance perception in the rat, *Univ. Calif. Publ. Psychol.*, 1930, 4, 233–239.
28. Shipley, W. C., An apparent transfer of conditioning, *J. General Psychol.*, 1933, 8, 382–391.
29. Spence, K. W., The order of eliminating blinds in maze learning by the rat, *J. Comp. Psychol.*, 1932, 14, 9–27.

30. SPRAGG, S. D. S., Anticipation as a factor in maze errors, *J. Comp. Psychol.*, 1933, 15, 313–329.
31. SWITZER, S. A., Backward conditioning of the lid reflex, *J. Exper. Psychol.*, 1930, 13, 76–97.
32. ——, Disinhibition of the conditioned galvanic skin response, *J. General Psychol.*, 1933, 9, 77–100.
33. TOLMAN, E. C., Purposive behavior in animals and men, New York and London: The Century Co., 1932.
34. ——, Sign-Gestalt or conditioned reflex? PSYCHOL. REV., 1933, 40, 246–255.
35. TOLMAN, E. C., AND HONZIK, C. H., 'Insight' in rats, *Univ. Calif. Publ. Psychol.*, 1930, 4, 215–232.
36. VALENTINE, H. M., Visual perception in the white rat, *J. Comp. Psychol.*, 1928, 8, 369–375.
37. WHEELER, H. W., AND PERKINS, F. T., Principles of mental development, New York: Thomas Y. Crowell Co., 1932.
38. WOLFLE, H. M., Conditioning as a function of the interval between the conditioned and the original stimulus, *J. General Psychol.*, 1932, 7, 80–103.

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THE PHYSIOLOGICAL BASIS OF LINGUISTIC DEVELOPMENT AND OF THE ONTOGENY OF MEANING. PART II

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IV. The Acquisition of Language

Section 1. Learning to Understand Words

A child's understanding of language does not begin with his recognition of words as grammatical units. He reacts rather to the spoken sentence as a whole and the situation in which it is spoken. Jespersen observes that "If the child learns the feat of lifting its arms when it is asked 'How big is the boy?' it is not to be supposed that the single words of the sentence are understood, or that the child has any conception of size; he only knows that when this series of sounds is said he is admired if he lifts his arms up; and so the sentence as a whole has the effect of a word of command. A dog has the same degree of understanding." [1]

The child regularly, though only vaguely, understands words before he can pronounce them. Preyer mentions a child 18 weeks old who on hearing 'tick-tack' spoken would look over at the clock; and this was long before he could say 'tick-tack' himself.[2] Romanes,[3] Major,[4] Bean,[5] and many others have reported similar cases. Charles Darwin relates of his little son, that "Before he was a year old, he understood . . . several words and short sentences. He understood one word, namely his nurse's name, exactly five months before he invented his first word *mum;* and this is what

[1] O. Jespersen, Language, its nature, development, and origin, 1921, p. 113.
[2] W. Preyer, The mind of the child, Part II, p. 69.
[3] G. J. Romanes, Mental evolution in man, 1893, p. 126.
[4] D. R. Major, First steps in mental growth, 1906, p. 290.
[5] C. H. Bean, An unusual opportunity to investigate the psychology of language, *J. Genet. Psychol.*, 1932, 40, 188.

might have been expected, as we know that the lower animals easily learn to understand spoken words." [6] Like Jespersen and Darwin, Preyer,[7] Kellogg,[8] and others have noted the familiar observation that dogs, horses and other domesticated animals often learn to understand words.

A child learns to understand some words, specially concrete nouns, because, as Jespersen says, "when the word is used, the object is at the same time pointed at." [9] If the home tabby is pointed out or placed in a child's arms and at the same time, *i.e.* while the child's motor exploration is going on, the sound stimulus 'cat' is given to the child's ear, the word will necessarily find outlet into the motor paths that will re-create the contours of the tabby. And of course the process is independent of a child's, or an animal's, being able or not able to articulate the word. Children's picture-books convey instruction in a similar way.

The child's acquisition of meanings of verbs is not much more difficult, for actions and processes can be readily pointed out to a child and the corresponding verb simultaneously spoken. But the process is more complicated for general and for abstract terms, and we shall not go into all the aspects of it. The process of learning word-meanings is never completed even in the adult, who continues to learn what words do or 'ought to' mean. It is, indeed, the very process by which linguistic meanings themselves evolve. To discuss this whole process would be to go into the psychology of discrimination and analysis, and much else. The physiological method which we have adopted, seems by no means inadequate to this task, as several of our later sections may indicate.

We wish next to show, quoting at length, and with his permission, from an unpublished paper by E. B. Holt, how the child's meanings develop greater precision along two lines, namely, towards more adequate connotation and towards more restricted denotation. As follows:—

[6] C. Darwin, A biographical sketch of an infant, *Mind*, 1877, 2, 294.
[7] W. Preyer, *op. cit.*, p. 68.
[8] W. N. Kellogg, Humanizing the ape, Psychol. Rev., 1931, 38, 169.
[9] O. Jespersen, *op. cit.*, p. 126.

It has been repeatedly observed that when a child first learns to call his papa by that name, he will call *any other man* 'papa'. This has been termed an early case of 'generalization'. But let us notice also that, for a few days or weeks, all of the child's other responses are likewise quite the same to any other man as to his own father. The little one is 'not afraid of strange men' for the reason that he is *not as yet aware of any difference* between his father and them. Clearly his word 'papa' denotes (for him) far too many men; and it connotes less, much less, than it should.

Precisely here it is that the physiological interpretation of consciousness departs from the traditional psychology, and leaves that psychology, as it seems to me, at a disadvantage. For the traditional view, committed as it is to the doctrine of 'sensationism' declares that the adequate stimulation of sense-organs produces 'sensations' in consciousness. The physiological interpretation, on the contrary, asserts that the adequate stimulation of sense-organs produces no consciousness whatsoever unless that stimulation excites muscular response:—re-creation.

The evidence is, in the case cited, that papa or any other man, as stimulus, excites the same responses (including the word 'papa') in the child. The word 'papa', therefore, means for the child no more (means in fact much less) than all men, as stimuli, have in common. The child may be re-creating, as distinguishing 'mamma' from 'papa', only mamma's one skirt and papa's (or any man's) two trouser-legs.

If the father frequently visits the nursery, the normal exploratory reactions of the infant will soon re-create, *i.e.* put into the young consciousness, features of the father which cannot be re-created when any other man is the stimulus; that is, features which would differentiate 'papa' from all other men. In this way the child becomes more fully and adequately conscious of that object, his father.

We must now look more narrowly at the above statement, that any man as stimulus excites the response 'papa'. Does the man as stimulus excite the response 'papa' directly? Is it the difference between mamma and papa as stimuli which determines directly whether the child shall respond 'mamma' or 'papa'? Without attempting to give a final answer to this question, we may cite introspective evidence to show that in many, perhaps in most, cases the answer is *no*. To judge from introspection, we do not name an object until *after* we have perceived it. If a chair on which

one is sitting collapses, it is not unusual to find that one has leaped out of the chair *before* one is conscious that the chair has collapsed. Or if, inadvertently, one has put the lighted end of a cigar into one's mouth, one is apt to be conscious first of holding the cigar off at arm's length and only later to taste ashes and feel pain in one's mouth. But the function of naming, like that of counting, seems to be of a different order: in some cases, at least, we seem unable to name, or to count, anything which we have not *already* perceived.

It would therefore seem that it is the child's perception (motor re-creation) of his mother's skirt which determines his saying 'mamma'; and his perception of his father's, or any other man's, two legs which sets off the response 'papa'. Now a motor act of re-creation is just as much a stimulus in the child's nervous system, as the physical presence of his mother or father can ever be, because the contraction of any muscle stimulates sense-organs (proprioceptors) in the muscle, which then send afferent (sensory) impulses back to the child's central nervous system. And the afferent pattern made up by these proprioceptive return impulses will differ as specifically from any other such afferent pattern, as the muscular act of re-creation which excites these impulses differs from another such muscular act of re-creation. So, if a child on *seeing* his mother speaks the word 'mamma', and on *seeing* his father says 'papa', it is because the proprioceptive return impulses from his motor re-creation of his mother's contours (perchance those of her skirt, merely) have become conditioned to his motor paths which say the word 'mamma', while a different proprioceptive return pattern (from exploring the contours of his father's two legs) has been conditioned to the motor paths which say 'papa'. Such conditioning will inevitably take place, since it is mostly when a person is present before him that he explores that person's contours and simultaneously hears that person's name; and hearing a name has very early been conditioned to speaking it. The part played by proprioceptive return currents in initiating new motor performances was described by William James in connection with the 'concatenation' of activities.[10]

Returning now to our question as to how the word 'papa' gradually acquires in the child's consciousness a more adequate connotation, or meaning, we find the answer at hand. The normal exploratory responses of the child will cause him to see his father more completely, and the father's dealings with the child will

[10] W. James, The principles of psychology, 1890, 1, 115–118.

develop further responsive habits (affectionate, playful, etc.) in the child. All of these, the growing 'father complex', besides being (by simultaneity) rather promiscuously conditioned to one another, will also (by simultaneity) be generally conditioned to speaking the word 'papa'; and this latter, in turn will be rather generally conditioned to these activities. Thus the word 'papa' acquires for the child an ever widening connotation, or meaning. It stimulates the child to ever more extensive and more diversified motor attitudes.

Step by step with this process the denotation of the word, 'papa', for the child, will become narrower. In the presence of a different man as stimulus, the child will also proceed to explore; and will infallibly run through those habits of exploration which he has learned on his father as stimulus. In so far as this other man is like the father, this 'perceiving his father *in* the stranger' will run smoothly, and the child will not perceive a difference. But the better the child knows his 'papa', the sooner will come the moment when this re-creating of the father will meet an obstacle and be checked. The little one cannot play with papa's beard on another man who has no beard, nor fit the curves of papa's rotundity to a lank figure that leads his eye and hand along straight lines and sharp angles. The proprioceptive return impulses are now, perforce, *not* altogether the same as those which are conditioned to the motor paths which say 'papa'. When the difference is great enough, the child will 'refuse' to say, that is, will not say 'papa'. The denotation of this word 'in the child's mind' has been restricted. Thus week by week as the connotation of the word 'papa' becomes more adequate, its denotation is reduced.

Enough has been said to enable the reader to work out, at his pleasure, the way in which, when a child has been told that 'pussy is a quadruped, doggie and mousie are quadrupeds, ponies, cows, and elephants are all quadrupeds', so many motor re-creations, all simultaneously stimulated by the one word 'quadruped', will in the execution interfere with one another until nothing is left but a blurry skeleton, an *image* not very different from a four-legged deal table with a very shadowy 'head' at one end and a 'tail' at the other. This is, of course, the 'composite photograph' view. And if any reader should hesitate to depart so widely from what David Hume said on the psychology of general ideas, one begs that he will put the matter to the test of introspection.

Section 2. Two Early Sources of Meaning

Learning to understand spoken words and learning to speak understandingly are of course closely connected processes. Except on a few elementary points it is not feasible to discuss them separately. In this section we shall consider two ways in which meaning accrues to sounds that were first spontaneously babbled.

The first case consists of spontaneous baby sounds which become words because older persons attach a meaning and themselves adopt the words so created. The mechanism here is the same as that by which an infant learns to cry in order to convey meaning; as was described in Chap. III, Sect. 3. In this way 'emotional cries' have given rise to various words, mostly of the interjectional order, some of which are recognized by lexicographers. Monboddo believed that words of this sort are numerous and important.

> It is therefore inarticulate cries only that must have given rise to language; and, . . . it appears at first sight very probable, that language should be nothing but an improvement or refinement upon the natural cries of the animal, more especially as it is evident, that language does no more than enlarge the expression of those natural cries. For such cries are used by all animals who have any use of voice to express their wants; and the fact is, that all the barbarous nations have cries, expressing different things, such as, cries of joy, grief, terror, surprise, and the like. The *war-cry* of the Indians of North America is well known to those that have been among them; and they have a cry, when they return from any expedition, by which they signify, before they enter their village, what success they have had.[11]

A mother or nurse often takes some babbled sound of the infant to be an effort to designate some object noticed by the infant, and therefore she fetches forward the object:—'Here's your *wu-wu*, dearie!' This practice if often indulged in gives rise to a rather extensive vocabulary of 'baby-talk', a language that is ephemeral and local to one baby's nursery, and that considerably retards the infant's progress in acquiring the

[11] Lord Monboddo (James Burnet), Of the origin and progress of language, Second edition, 1774, Vol. I, p. 475.

actual parental tongue. But some words which have originated in this way are very far from being either ephemeral or local, and of these the classical examples are the words 'mamma' and 'papa'. As Jespersen has remarked (*cf.* Chap. II), "the labials, *p*, *b*, and *m*, are early sounds if not the earliest" [12] which an infant utters; and of vowels, the 'open' (*o*, *a*) are the earlier. This seems to be true of infants belonging to any race. "I am fully persuaded", wrote de Brosses, "that were a child left entirely to himself without hearing either a human or an animal voice, he would begin his use of language with the syllables Papa and Mama . . . the easiest vowels and consonants because they are essentially those in which the simplest movements of the lips are involved." [13] Now the mother (or nurse) and father are the persons in closest contact with the new born baby, and they, eager to find that the little one 'recognizes' them, add as 'meaning' of the first babbling—He means me! The result of this egocentric delusion has been remarkable: in almost every known race, ancient or modern, the familiar names for mother, father, and nurse are one or other of the following first sounds, ma, ba, pa, na, da, ta; with the infantile reduplication. Possibly other factors have contributed towards a convergence; in any case 'mamma' is the most widely used name for mother, and 'papa' for father. But, de Brosses states, "The Georgians and Iberians say *Mamao* for father . . . among the inhabitants of the East Indies, the word *mama* signifies man or father." [14] We may add that in Persian Urdu the word *mámá* denotes a nurse or maid-servant; the word *mámú* denotes maternal uncle. In the Panjabi language the word *mámá* denotes the maternal uncle; whereas *mumma* denotes the maternal breast.

The fact that in so many languages the word for mother contains the syllable *ma* and for father the syllable *pa*, has led some philologists to suspect a single remote linguistic source. They may well be called two fundamental roots but,

[12] O. Jespersen, Language, its nature, development, and origin, 1921, p. 105.

[13] C. de Brosses, Traité de la formation méchanique des langues, 1765, Vol. I, pp. 233–235.

[14] *Ibid.*, p. 238.

as Jespersen warns us, it is "wrong to use them as evidence for original kinship between different families of language and to count them as loan-words. . . . The English *papa* and *mam(m)a*, and the same words in German and Danish, Italian, etc., are almost always regarded as borrowed from French; but Cauer rightly points out that Nausikaa (Odyssey 6. 57) addresses her father as *pappa fil*, and Homer cannot be suspected of borrowing from French." [15] Words that have originated in any such way as have mamma and papa, call for great circumspection on the part of philologists. And it is a question how many such words there may be.

De Brosses [16] drew up a long list of 'primitive keys' or 'generic roots', each one of which is a sound (usually a vowel and consonant) which, given the normal human body and vocal apparatus, de Brosses believed is as much bound to have a certain meaning universally attached to it as are the syllables *ma* and *pa* (which are included in his list). For each such root he gives a considerable number of illustrations, drawn from widely separated languages. The sound ST, for instance, occurs in many languages in words which signify stability, or some aspect of that idea. In some cases he suggests how these quasi-inevitable connections between sound and meaning may have come about; but on the whole he is more concerned to demonstrate the existence of these connections, than to explain their origin. If these 'roots' appeared to be onomatopoetic sounds, they would present no mystery; but most of them in no way suggest onomatopœia. It seems possible that these connections between sound and meaning may involve, in addition to sound production, inevitable interrelations between organism and environment; in ways roughly analogous to the cases of 'mamma' and 'papa'. Whatever the emotions felt by grammarians or even semeiologists toward de Brosses' 'generic roots', we believe that he has raised a question which, if solved, may have important bearing on the relation between the holophrastic and the root stages in the development of languages. It is

[15] O. Jespersen, *op. cit.*, 159.

[16] *Op. cit.*, Vol. I, pp. 224–288; Vol. II, pp. 194–405.

pleasant to record that tentative studies along cognate lines are being attempted by a few psychologists.[17]

The second way in which meaning accrues to merely babbled sounds is the long familiar phenomenon called onomatopœia. W. D. Whitney describes onomatopoetic words as "those in which the attempt had been made in a rude way to imitate the sounds of nature: as when the *cuckoo* and the *pewee* and the *toucan* were named from their notes; or as in some of the descriptive words like *crack* and *crash*, *hiss* and *buzz*, which are by no means all old, but have been made, or shaped over into a pictorial form, within no long time. We call such words *onomatopœias*, literally 'name-makings', because the Greeks did so: they could conceive of no way in which absolutely new language-material should be produced except by such imitation." [18] When the child hears an animal or an object produce a characteristic sound, it tends to reproduce that sound through the mechanism of auditory-vocal reflex-circles (as explained at the end of Chap. II). During the lalling period, reflex-circles are developed such that when the infant hears a sound he at once repeats it as closely as his vocal apparatus permits. After thus exercising his entire gamut of possible vocalizations, whether his mother says *mamma*, a dog says *bow-wow* or *bark*, a wheelbarrow says *creak*, or a waterfall says *roar*—the infant repeats ('imitates') the sound. Such sounds come to 'mean' the objects or processes themselves, and so become 'words', because when the sound is heard the object or process emitting it is near enough to be explored and re-created by the child's sensori-motor organs. Each sound acquires its meaning in a motor negotiation ('experience') with the object that emits it, and the sound will have the same meaning if it is spoken as a 'word'. 'Bow-wow', 'Ding-dong', and 'Yo-heave-ho' are all, we believe, cases of sounds with 'natural meaning'.

Preyer rightly regards 'the onomatopoetic attempts of

[17] *E.g.* E. M. von Hornbostel, Laut und Sinn, *Sprachwissenschaftliche und andere Studien* (für Karl Meinhof), 1927, pp. 329–348; A. Wellek, Der Sprachgeist als Doppelempfinder, *Zsch. f. Aesth. u. allg. Kunstwiss.*, 1931, 25, 226–262.

[18] W. D. Whitney, The life and growth of language, 1875, p. 120.

children as simply a sort of imitation'.[19] Such imitation of animal cries and natural sounds (such as thunder) is of course inexact, and varies from child to child. It is not surprising that onomatopoetic names for the same object are slightly different in different languages:

> Thus to English *cockadoodledoo* corresponds French *coquerico*, German *Kikeriki*, and Danish *kykeliky*, to E. *quack-quack*, F. *cancan*, Dan. *raprap*, etc.[20]

To this list we may add the Panjabi *Kukroon-ghoon* and *ghan-ghan* as characteristic onomatopoetic words for rooster and duck respectively. Conforming to current superstition, Ernest Weekley suggests that 'ancestry' may account for the individual variations in each child's onomatopœia:

> The exact form might be conditioned by his ancestry, for it is a curious fact that babies of different nationalities interpret animal cries variously. The *cock-a-doodle-doo* of the English chanticleer is to a French ear *cocorico*, to a German *Kikeriki*. That the more clearly defined note of the cat offers fewer opportunities for variation is evident from the fact that the Chinese for cat is *miau*, a form, by the way, much more acoustically accurate than our conventional, literary *mew*. The feline murmur of pleasure is less easily represented by spelling, and there is a wide difference between our *purr* and the equally expressive French *ronron*.[21]

In view of what we know about the reflex-circle, we feel that no ancestry is in question; unless, indeed, the ancestry of the chanticleer and the cat.

Section 3. The 'Little Language', and on Interpreting It

In the lalling period the infant practices its 'primordial babblings' until its elders almost come to consider it a little engine of torment. Yet it needs to practice these sounds well, for they comprise nearly all the sounds that it will later use.[22] As it grows on to childhood it is incessantly active,

[19] W. Preyer, The mind of the child, Part II, p. 100.

[20] O. Jespersen, Language, its nature, development, and origin, 1921, p. 150.

[21] E. Weekley, Adjectives—and other words, 1930, p. 84.

[22] *Cf.* W. Stern, Psychology of early childhood, 1924, p. 145; M. M. Nice, A child's attainment of the sentence, *J. Genet. Psychol.*, 1933, 42, 219.

new reflex conditionings (which no outsider can keep account of) are steadily going on, and soon its behaviour, vocal and other, baffles the comprehension of grown-ups. It makes earnest but often incomprehensible vocal appeals to its elders, and in fact acquires a strange 'little language' of its own. As Jespersen says:

> The 'little language' which the child makes for itself by imperfect imitation of the sounds of its elders seems so arbitrary that it may well be compared to the child's first rude drawings of men and animals. A Danish boy named Gustav (1.6) called himself [dodado] and turned the name *Karoline* into [nnn]. Other Danish children made *skemmal* into [gramn] or [gap], *elefant* into [vat], *Karen* into [Gaja], etc. A few examples from English children: Hilary M. (1.6) called Ireland (her sister) [ani], Gordon M. (1.10) called Millicent (his sister) [dadu]. Tony E. (1.11) called his playmate Sheila [dubabud].[23]

The child's imitation of its elders is by no means the only factor contributing to the little language, as is shown by the fact that children of about the same age growing up together, especially if they are left a great deal by themselves, often develop an astonishing jargon of their own which can hardly be called any longer a 'little' language. Twins are very apt to do this. Jespersen records a case of twin boys about five and a half years old, who had been left very much to shift for themselves. "When they were by themselves they conversed pretty freely and in a completely unintelligible gibberish, as I had the opportunity to convince myself when standing behind a door one day when they thought they were not observed." [24] Such children are found to be somewhat handicapped for learning their normal mother tongue.[25]

Mispronunciation is of course a feature of the 'little language'. The very young child lisps, and there are other sounds than *s* as well as combinations of sounds, employed in language, which the child finds difficulty in articulating.[26]

[23] O. Jespersen, *op. cit.*, p. 106. The notation of age, *e.g.* (1.6), means 'in the seventh month of the second year'.

[24] *Ibid.*, pp. 185–186.

[25] E. J. Day, The development of language in twins, *Child Development*, 1932, 3, 179–198.

[26] O. Jespersen, *op. cit.*, Chap. V, §§ 3–6.

In the case of the writer's son, it was observed that at the age of about two years the sound *gu-gu* was his nearest approximation to the word *mango*. Later on he used the same sound to indicate any kind of fruit. This sound was adopted by the older members of the family in imitation of the child, and remains to this day the family word for any kind of fruit; although the child is now old enough to pronounce almost any word in the English language perfectly.

One is tempted to wonder whether this childish practice of words imperfectly pronounced may have a cumulative (and degrading?) effect, as one generation succeeds another, on the standard adult pronunciation of the language, and even on the forms of words. Some philologists, we believe, have held it responsible for sound changes. But this is a problem for the phonetician, rather than the psychologist.

Another characteristic of the 'little language' and indeed of children's speech (and thought) in general, is that tendency to 'gross generalization', as it has been called, of which we have already seen a case in the infant's naming of any man 'papa' and of any woman 'mamma' (Chap. IV, Sect. 1). At a time when very inept 'explanations' of this phenomenon were being offered, J. M. Baldwin came very close to the actual physiology of the matter:

> It is only partially true that the concept arises from the percept at all. It is rather true that the two arise together, by the same mental movement, which is apperception or motor synthesis. . . . He [an infant] reacts to it [a presentation A in consciousness], and so stands ready to react to it again. This readiness is his *expectation*—the only tendency he has to a definite reaction; and as the only one, it stands ready to 'go off' on any kind of stimulus which is locally near enough to discharge that way. Whatever then actually does happen is at first reacted to as A, and remains A, by this active confirmation [re-creation?], if it is possible for the child's consciousness to keep it A.[27] . . . It is evident that the 'general' or 'abstract' is not a content at all. It is an attitude, an expectation, a motor tendency.[28]

[27] J. M. Baldwin, Mental development in the child and the race (2nd. ed.), 1895, p. 326.

[28] *Ibid.*, p. 330.

It would be unjust to imply that Baldwin's theory of the mind was exclusively physiological and motor, but his pointing to motor pathways previously organized and now liable to be set off by a range of stimuli beyond those to which the particular response is specific, was a long step forward. It is not merely the 'gross generalizations' of children that are to be explained in this way, but many profoundly important phenomena of the adult mind—'stereotypes', which are the often equally gross generalizations of men and women, magic and other forms of superstition, fictions or the 'as-if' notions, and so on. So far as the generalizations of children go, we have sufficiently described the physiological process involved, in Sect. 1 of this Chapter.

The interpretation of children's early linguistic efforts is difficult. And in studies of infantile linguistics the mistake has often been made of reporting sounds and listing the words used by a little child without observing *at the same time* the whole behaviour of the child in the concrete situation which evoked these verbal responses. What a child means by a word is revealed only by what he is doing when he speaks the word.

The same is true of the sounds made by animals. Wallace Craig, in his extraordinarily instructive studies of the behaviour of pigeons, has shown that the sounds made by a pigeon become significant only when studied in connection with the pigeon's simultaneous 'bowing, strutting, bristling of feathers', etc.[29] And it may be questioned whether the scientific study of languages has not suffered by too exclusive attention to written words and word-sounds, apart from their *reference;* that is, apart from the concrete situations in which they were used and the concrete behaviour of the persons who used them. As W. L. Graff has remarked:

> Especially historical linguists, with their dependence upon written documents and their desire for tangible and clear-cut data, are liable to study sounds instead of words. Etymologizing is only too often nothing but an application of phonetic laws or formulæ.

[29] W. Craig, The voices of pigeons regarded as a means of social control, *Amer. J. Sociol.*, 1918, 14, 88.

It is forgotten that such work is mere pioneer work, destined to clear the way for more comprehensive solutions. That is why many historical phonetic studies are so dead and devoid of human interest. . . . Is it to be wondered at that linguist and psychologist are so often at variance? [30]

Anyone who studies the speech of children is prone to impute to the child the same 'meaning' which the word or sound uttered by the child has for the observer himself. This is the fallacy of enelicomorphism. But in this way the student will be almost invariably misled. M. W. Humphreys writes of a little girl 18 months old:

Until her eighteenth month she employed 'No' for both Yes and No, and then she substituted 'Mam' (from Yes, Mam) for 'No' in the sense of Yes, and retained it till she was two years old, using 'Yes' only when it was specially suggested to her.[31]

The infant's mere lalling of 'ma-ma-ma-' has not a vestige of meaning for the infant, and yet, as we have seen, the mothers of the world from prehistoric times have supposed themselves to be by this sound recognized and called by name.

In order to find out what denotation and connotation a child's use of the word 'mamma' implies, one must find that action, presumably on the part of the mother, which when performed will leave the child satisfied, that is, quiescent and expecting nothing further. It is with expectation, that is with muscles set for some action but that action suspended, that the child calls for 'ma-ma'. And this suspended motor expectancy *is* the meaning, on each occasion, of the child's word. It means that for which he wants his mamma; and this meaning is not in the word but in that situation in which the child's readiness for action waits suspended. Hence, as we have said, one who records only a child's vocalizations will never discover the significance of the child's words. The child calling to his 'ma-ma' is in every way analogous to our previous illustration of the dog that dropped the ball at

[30] W. L. Graff, *Language and languages*, 1932, p. 99.

[31] M. W. Humphreys, A contribution to infantile linguistic, *Trans. Amer. Philol. Assoc.*, 1880, 9, 10.

a stranger's feet and then looked up waiting for the stranger to throw it. So here, if in response to the word 'ma-ma', the breast or the nursing-bottle is proffered and the child's suspended motor set at once goes into action, with no trace of suspense remaining, and irrespective of whether the mother or someone else did this proffering, then we are justified in concluding that the child meant only 'milk' and no more. The physical, mental, social and other aspects of the mother were entirely absent from this meaning.

This method of interpreting child language is amply confirmed and justified, as it seems to us, by the facts which we shall consider in the following two sections.

Section 4. The Action-Content of Words; the Holophrase

Practically all of the words used by children have a decided 'action content', some concrete objective reference, and above all a reference to the child himself. That the young child has few, if any, abstract words, has been demonstrated by W. E. Bohn,[32] W. Boyd,[33] J. Drever,[34] and M. M. Nice.[35] Chamberlain,[36] Tracy,[37] Dewey,[38] Binet[39] and others have shown that a child's words refer to his own behaviour as well as to that of others.

The child's earliest words are not the grammarian's 'parts of speech', but word-sentences. This fact has been ignored by some observers, who have gathered data to show the percentage of 'nouns' etc., in an infant's early vocabulary; whereas the words of little children, whatever they may sound like, do not function as nouns, verbs, or other parts of speech. Careful observation of child behaviour has shown that the child's so-called 'nouns' or 'verbs' are actually holophrases, that is, are in fact sentences. J. F. Markey states that

[32] W. E. Bohn, First steps in verbal expression, *Ped. Sem.*, 1914, 21, 578–595.

[33] W. Boyd, The development of a child's vocabulary, *Ped. Sem.*, 1914, 21, 95–124.

[34] J. Drever, A study of children's vocabulary, *J. Exper. Ped.*, 1915, 3, 182–189.

[35] M. M. Nice, Speech development of a child from eighteen months to six years, *Ped. Sem.*, 1917, 24, 204–224.

[36] A. F. and J. C. Chamberlain, Studies of a child, *Ped. Sem.*, 1909, 16, 62–103.

[37] F. Tracy, The psychology of childhood, 1909, p. 150.

[38] J. Dewey, The psychology of infant language, PSYCHOL. REV., 1894, 1, 63–66.

[39] A. Binet, Perceptions d'enfant, *Rev. Phil.*, 1890, 30, 582–611.

"there is also practically universal agreement upon the fact that the first symbols of the child are in reality word-sentences designating action and subject or object, or all three at once"; [40] and that "the classification of a child's vocabulary upon the basis of the adult parts of speech is a highly arbitrary and fictitious process. These first so-called nouns, as Dewey (1894) and others have pointed out, are in reality verbal-adjectival-nominal or nominal-adjectival-verbal symbols, and the like. In short, they are action words and word-sentences, as we have seen, and are often accompanied by the appropriate action on the part of the child." [41] H. Lukens emphasizes the same point: "To classify such child-words by the adult distinctions of the parts of speech . . . is of course to be misled by very superficial considerations. It does not seem possible to classify a child's words until he uses all of the parts of speech." [42] Sully also observed that the first words of a child are as a rule whole-sentence words.[43] Charles W. Waddle says that "a single word conveys a variety of meanings, depending upon intonation, inflection, accompanying gestures, facial expression, pantomime, and similar factors. The pronoun 'me' often performs the function of such sentences as 'take me up', 'I want to ride', 'give me the book', 'let me go with you'; the preposition 'up' may mean at one time, 'I want you to give me my ball', or again, 'I want you to put me up in my chair'." [44]

In connection with the theory of 'recapitulation' as also with the general theory of language development, it is interesting to note that the holophrastic use of sounds is found not only in children, but also among primitive men and in primitive languages. This fact was discovered by Europeans early in the eighteenth century, if not indeed much earlier, and has still, we believe, not received its due recognition in linguistic science. Lord Monboddo, writing in 1774, de-

[40] J. F. Markey, The symbolic process, 1928, p. 50.

[41] *Ibid.*, p. 54.

[42] H. Lukens, Preliminary report on the learning of language, *Ped. Sem.*, 1894, 3, 424–460.

[43] J. Sully, Studies of childhood, 1895, p. 171.

[44] C. W. Waddle, An introduction to child psychology, 1918, pp. 166–167.

scribed the languages of some of the North American Indians as follows:

> With respect to syntax, they appear to have none at all; for they have not prepositions or conjunctions. . . . In short, they have not, so far as I can discover, any way of connecting together the words of their discourse. Nor is this a peculiarity in their language; but it is the same in the languages of the Galibi and Caribs, ... Those savages, therefore, tho' they have invented words, use them as our children do when they begin to speak, without connecting them together; from which we may infer, that syntax, which completes the work of language, comes last in the order of invention, and perhaps is the most difficult part of language. It would seem, however, that persons may make themselves understood without syntax. This I think can be done no other way but by the arrangement of words (which is a considerable part of the syntax in modern languages that have not cases), by accents or tones, or by gestures and signs. The Hurons, and I believe all the barbarous nations, have a great variety of tones; they have also much action in their speaking; and there can be no doubt but that the position of the word will commonly determine what other word in the sentence it is connected with.[45]

Monboddo gives the following instances of holophrastic words from the languages of these Indians:

> . . . they have a great many words, which are so many sentences. Thus, they express by one word, *There is water in the bucket;* by another word, quite different, *There is a great deal of water;* by a third, different from either, *You have overturned the water in the fire.* But by one and the same word they express, *Thou* shalt be *very glad of it*, and *Thou* art *very glad of it.* Their verbs commonly express the action with the subject [= object] of the action; and but very few denote the action simply by itself. Thus, there is no word which signifies simply to *cut*, but many that denote *cutting fish*, *cutting wood*, *cutting cloaths*, *cutting the head*, *the arm*, etc. In like manner, they have no word that denotes the simple idea of *giving;* but there are two or three pages in our author's [G. Sagard's] dictionary filled with words signifying *to give* different things. This again multiplies their words so much, that, if it were not for the reason above mentioned [*i.e.* p. 533, "if their sphere of life were not

[45] Lord Monboddo, Of the origin and progress of language, 1774, vol. I, pp. 537–538.

very narrow"], their language could not serve the ordinary purposes of life.[46]

The sphere of life of primitive men may be relatively narrow, yet they have their enterprises and responsibilities, and their cumbersome holophrastic vocabularies contain a prodigious number of words. The holophrastic efforts of little children belong to a still more primitive phase, and here we are able to observe in all clearness a phenomenon which is present but less conspicuous among savages, and not quite extinct even in the most polite conversation: that a word with many meanings depends on intonation and gesture to make the intended meaning explicit. These latter are functionally a part of the word. Clearly this phenomenon is a vestige of the 'whole-body language' mentioned in Chap. III, Sect. 3. Preyer observes that "it is of the greatest importance for the understanding of the first stage of the use of words in their real significance, after the acquirement of them has once begun, to observe how many different ideas the child announces by one and the same verbal expression. Here are some examples: Tuhl (for Stuhl, chair) signifies— 1. 'My chair is gone'; 2. 'The chair is broken'; 3. 'I want to be lifted into the chair'; 4. 'Here is a chair'." [47]

The physiological explanation of holophrastic words is simple. Any sound that a person can make (whether he speaks, whistles, snaps his fingers, or rings a bell) can be conditioned in himself and in others to any amount of action. That sound is then a holophrastic signal, or symbol. If it is a spoken sound, it is called a holophrastic word, or holophrase. It is by *analysis* of the activity which the symbol excites that we arrive at the various 'parts of speech' and their syntactic order, that is, the sentence. This analysis of human action, if we consider it attentively, can never be carried to completion: any word whatsoever remains to some extent a holophrase. And the majority of words, in every language, remain grossly holophrastic.

[46] *Ibid.*, pp. 534–535.

[47] W. Preyer, The mind of the child, Part II, pp. 95–96.

Section 5. The Child's Speech is Egocentric

As every reflective mother well knows, the behaviour, speech, and thought of a child are egocentric: they all center round his own needs and impulses. From the physiological point of view, this self-centeredness is to be expected; and recent observations on children well illustrate it. Of these, probably none are more illuminating than the well-planned and careful observations of Jean Piaget and his associates, made at the Maison des Petits in Geneva.

For a period of about one month, the words and all accompanying action of several children were very minutely and *intelligently* observed, and carefully recorded. The records show, among other things, that even when a child was apparently addressing another child, he was as a matter of fact talking to himself, and seldom if ever even paused for an answer. This kind of talk Piaget calls 'collective monologue', a variety of egocentric speech. After a simple mechanical device had been explained to one child, he was asked to explain the mechanism to another child. The explanation and the demeanour of both children were carefully watched and recorded. The first child behaved as if he were explaining the mechanism to himself, and paid no thought to making himself understood by the other. In the words of Piaget, "the child of 6 to 7 still talks to a great extent for himself alone, without trying to gain the attention of his hearer."[48] Such naively egocentric speech drops off markedly at about the seventh year. But Piaget has shown that grossly egocentric thought and speech predominate in the early years of the child.

The researches of Piaget have been abundantly confirmed by other investigators. M. E. Smith, who has collected data on the conversation of hundreds of children, writes:

> In looking over the conversations collected it is evident that, in the strictest use of the word, very little of the younger children's talk is conversation. It rather approaches monologue, being a running commentary on the child's own actions, as in the case of Boy 30, aged two years, eleven months: 'I am making cake. . . .

[48] J. Piaget, The language and thought of the child, 1926, p. 100.

I'm going. No, I got. I got. My finger can get in. Just fell. I make something out of sand. Some splashed out', or, as an expression of his desires, when the same child goes on, 'Let me pat. I want some more blocks. Can I have that track? Won't go. That's enough. I want some. I want some more. I want vinegar jar'.[49]

Several other studies have been stimulated by Piaget's researches. In the year 1929, Rugg, Krueger, and Sondergaard studied the conversations of kindergarten children. Out of the three thousand remarks recorded, about 40 percent revealed not self-centeredness but self-assertiveness, while a negligible percentage of .25 contained ideas of self-depreciation. These observations confirm Piaget's view that a kindergarten child is eminently egocentric.[50] Vygotsky and Luria also confirm Piaget as to the egocentric character of child language. They assert, moreover, that egocentric speech does not really disappear at maturity: such part of it as is driven under cover (and not all of it is, by any means) continues as internal speech, which serves the same purpose.[51]

Section 6. The Sentence-Building Activity of the Child

Clare and William Stern have drawn the following distinction between a word and a sentence: "The speech units of the child belong to no single word-class, since they are not single words but sentences. For a word is the expression for a unitary content of consciousness; a sentence, on the contrary, is the expression for a unitary (completed or to be completed) attitude with regard to the content of consciousness."[52] The holophrastic use of words appears to be the child's first effort at making a sentence. But the holophrastic use of words must not be confounded with true syntactic activity, which involves the integration of words in a definite and

[49] M. E. Smith, An investigation of the development of the sentence, etc., *Univ. of Iowa Studies: Studies in Child Welfare*, 1926, Vol. III, No. 5, p. 21.

[50] H. Rugg, L. Krueger, and A. Sondergaard, Studies in child personality: I. A study of the language of kindergarten children, *J. Educ. Psychol.*, 1929, 20, 1–18.

[51] L. S. Vygotsky, and A. R. Luria, The function and fate of egocentric speech, *Ninth Internat. Cong. Psychol., Proceedings and Papers*, 1929, publ. 1930, pp. 464–465.

[52] C. and W. Stern, Die Kindersprache, 1907, p. 164.

articulate relation. The effort of the human race to invent symbols (words) which will stand for the various moments of experience, and then to combine those symbols (the task of 'syntax') so as at all adequately to represent, and so to communicate, the pattern of experience, has been a mighty enterprise; and it is by no means fully accomplished even yet. But a very intricate system of conventions has grown up, and here, naturally, the child learns his way about only slowly and painfully.

Lord Monboddo might have said of the child the same that he says of the savage in the following paragraph:

> Let us take, for example, the verb signifying to *beat*. There is first the action of beating; then the agent or person who beats; then the person or thing which suffers, or is beaten; and, lastly, there is the manner of beating, whether quickly or slowly, severely or gently, etc. But all these exist together in nature; and therefore the savage considers them all in the lump, as it were, without discrimination; and so forms his idea of the action; and according to this idea expresses it in words. Whereas, in languages formed by rule, all those things are expressed by separate words, or by variations of the same word, if that can be conveniently done. Further, there are some necessary adjuncts of the action, such as time. This too, though inseparably joined with it in nature, accurate abstraction separates, and expresses either by a different word, or by a certain variation of the same word: But this the savage likewise throws into the lump, and expresses all by the same word without variation, or by a word quite different. There is also the disposition or affection of the mind of the speaker, with respect to the action affirming or denying it, commanding it, or wishing it. These dispositions, in regular languages, are expressed, either by separate words, or by a variation of the word denoting the action; whereas, in the languages we speak of, they are either not expressed at all, or by a word altogether different. This will produce a further increase of words not necessary: For as there is no word expressing the action simply by itself, if there be the least change in any circumstance of the action; nay, if there be but an alteration in person, number, or time, or in the disposition of the mind of the speaker with respect to the action, there must be a new word. For, as they have no ideas of those circumstances separate from the action, they can have

neither separate words to express them, nor variations of the same word, even if they knew that great secret of artificial languages, I mean inflection.[53]

The syntactic activity of the infant is a slow and laborious process; and does not proceed far without social cooperation. In the case of little Frans (1.7), reported by Jespersen, the process took the following form. He was accustomed to say a single-word 'vand' whenever he wanted water. One day "his mother said: 'Say please'—and immediately came his 'Bebe vand' . . .—his first attempt to put two words together." [54] Jespersen goes on to say: "Later—in this formless period—the child puts more and more words together, often in quite haphazard order: 'My go snow' ('I want to go out into the snow'), etc. A Danish child of 2.1 said the Danish words (imperfectly pronounced, of course) corresponding to 'Oh papa lamp mother boom', when his mother had struck his father's lamp with a bang. Another child said 'papa hen corn cap' when he saw his father give corn to the hens out of his cap." [55]

With the further growth of the infant's symbolizing activity, more symbols are strung together in a sort of loose sentence, though the syntactic structure is still rudimentary; as has been described by J. F. Fenton:

In many of my child's first sentences I could see quite clearly that each word came forth as a separate mental act; the sentence did not represent one total idea, thought of all at once, but a series of details, noted and expressed one at a time, as one might point out and name one by one the separate elements of a picture, without first realizing its total significance at all. For instance, the longest sentence of his first eighteen months, uttered soon after seeing his father climb into an automobile with another man and drive away, consisted of the words, 'daddy, school, man, auto'. But the inflection of his words, the pauses between, the thoughtful expression accompanying each word, all pointed to the conclusion that his idea was put together bit by bit, like a mosaic, out of separate short and

[53] Lord Monboddo, Of the origin and progress of language, 1774, vol. I, pp. 528–530.
[54] O. Jespersen, Language, its nature, development, and origin, 1921, p. 134.
[55] *Ibid.*, pp. 134–135.

simple mental acts. It has less the sound of one connected sentence than of four brief exclamations, 'Daddy!' 'Kool!' 'Man!' 'Atto!' [56]

The age at which such sentence-construction begins, varies with individual children. To quote James Sully:

> The age at which it is first observed varies greatly. It seems in most cases to be somewhere about the twenty-first month, yet I find observers among my correspondents giving as dates eighteen and a half and nineteen months; and a friend of mine, a Professor of Literature, tells me that his boy formed simple sentences as early as fifteen months. . . . In the case of one child about the age of twenty-three months most of the sentences were composed of two words, one of which was a word in the imperative. . . . M's first performance in sentence-building (at eighteen and a half months) was, 'Mamma, tie', *i.e.* 'tie gloves'.[57]

Of a case under observation, K. C. Moore writes, that "the first sentence was uttered in the sixty-sixth week. It contained but two words, 'papa gone', and was the product of much previous practice on the part of the child, who had made many trials before he was able to pronounce successively the sounds therein contained".[58]

Sometimes a little child will hear a sentence spoken and will later repeat it merely as a phonetic unit, not at all realizing the distinct meanings of the several component sounds. This, of course, does not indicate progress toward understanding syntax. Another way in which a rudimentary sentence-building is simulated, as Jespersen warns us, is through 'echoism'. As a child repeats any sounds, reflexly or 'imitatively', he will often repeat ('echo') the last couple of words of a sentence which is addressed to him or spoken in his hearing, and these words may sound like a childish sentence. Jespersen gives the following illustrations:

> 'Shall I carry you?—Frans (1.9): Carry you.
> Shall Mother carry Frans?—Carry Frans.
> The sky is so blue.—So boo.
> I shall take an umbrella.—Take rella.' [59]

[56] J. F. Fenton, Practical psychology of babyhood, 1925, p. 137.
[57] J. Sully, Studies of childhood, 1895, pp. 171–172.
[58] K. C. Moore, The development of a child, *Psychol. Monog.*, 1896, No. 3, p. 131.
[59] O. Jespersen, *op. cit.*, p. 135.

It is evident that the progress from undifferentiated holophrase to articulated sentence is a progress in becoming specifically conscious of what one means, in analyzing that vague desire which the holophrase sought to express, into successive steps. This corresponds exactly to the general progress of thought, always towards analysis, in both an individual and a society. It also exactly corresponds to that exploration and re-creation of reality by our motile organs, our antennæ as it were, which we have held to be the very basis of all awareness. The child proceeds, slowly and with faltering steps, to explore his environment, and as he does so his words become articulate. We are convinced that in this conception of thought as a journey of exploration, lies the secret of syntax: "papa—hen—corn—cap"; "Daddy —school—man—auto"; Writer—tell—syntax—more—next—Chapter.

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FREQUENCY, DURATION AND RECENCY VS. DOUBLE STIMULATION

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"To know the psychological law, you must first master (1) those hundred and some pages in which Wundt describes the morphological features of the brain, its gross or topographical anatomy, and then you must master (2) his philosophy of the dimensions of feeling. Then you will put the two together and understand (3) how the mind can change the body with the wonderful result that the body can survive in almost any geographical environment although Nature did not fit the human species for it directly. It is the mind through which the body *learns* to conquer the Earth." With such words an older student initiated me, forty years ago, into psychology. Apparently there were two premises (1, 2) and (3) a conclusion. It seemed the height of logic. Notwithstanding, I discovered after several months of painful study that these two premises possessed no mutual relation, no common element, and that the conclusion concerning *learning* was a vapor.

My older friend referred me to Wundt's book because we were students in a university at some distance from Wundt's university, so that I could have access to his teaching only through his books. The fact that a part of the brain was called (in the pages of that book) the 'hippocampus', reminded the classically trained student of the chariot of Poseidon, but of nothing else, for the most strenuous search in that great book of Wundt failed to disclose the remotest reference to that division of the mind which supposedly resided in and mysteriously modified this hippocampus of the brain. My conclusion from the two 'premises' was, not that I now knew something about psychology, that is about *the science of learning*, but that the great book was the most awful conglomerate of unrelated stuff.

Quite recently I opened a book published in 1932, one of the dozen which are likely to appear every year on psychology, general or applied. From that early scholastic experience it may be inferred without my saying it, that, whenever I open a new psychology book, I immediately attempt to see how the subject of *learning* is treated in it—what the author teaches about the 'law' or 'laws' of *learning*. I was disappointed again, as I am nearly always in such a case. Driving in the ruts of the road, the author said that the laws of learning were the three of Frequency, Duration and Recency, with F, D and R capitalized. This time I felt not reminded of Poseidon, but of the prince Paris who had only one apple while three goddesses were contestants for the beauty prize.

In these latter days we classify psychology as a science. In our textbooks we fail to make clear to our students what is meant by a scientific law.

During those same student days I listened to the lectures of a great teacher on theoretical physics. His lectures were clear and full of meaning. This happened to be one of the two men whom the world now regards as the greatest living physicists. When he spoke of a law, he immediately wrote an equation on the blackboard. The equation contained at least two variable quantities (sometimes more), such as pressure and volume, or radial acceleration (*e.g.*, of a particle of ocean water) and tangential velocity of the earth in its orbit. An equation with one variable alone ('t' or duration) was never offered as a law. It also contained so-called constants. He made it clear that the variables of the law had to be *defined* by description just as one teaches a child by description what the word 'duration' or 'length' or 'color' means; that the constants were numbers which could be (with approximation) *discovered* only by experiment; and that the formula of the equation had to be *invented* by a keen intellect. Never did he teach us such nonsense as one finds in some psychology books, say, that the normal recital of a half-minute speech for the sake of learning it was learning by 'the law of duration' and that a normal practice of three

minutes devoted to the same task was learning by another law, by 'the law of frequency'; or that leaving a kettle of water on the stove for one minute was 'heating by the law of duration' and that reaching for it and putting it back there for a second minute was 'heating by the law of frequency'. Nor did he tell us that 7 minus 3 was 4 'by law'. He told us that it was so *by definition.* It dawned upon me, studying also psychology, that the law of recency was (1) no law and (2) no concept of learning, but that it was a descriptive definition of *forgetting*, if properly expressed in words.

A number of years afterwards I had to teach psychology to college students. I conceived of psychology as the science of learning; I conceived of learning as a conductivity change (or call it resistance variation) somewhere in the nervous system. I warned my students against the notion that the gross anatomy (the 'hippocampus anatomy') could have any except the most remote causal relation to any law of learning. I taught them that the actual nervous system, as we knew it histologically, could determine an action, not as an exclusive result, but only by letting a dominant flux go toward the 'active' motor point and *innumerable minor fluxes toward all other muscles;* but that nothing prevented us from *agreeing* to speak of the dominant flux as 'the' one nervous current, provided we did not forget that this was a convention. I told them that everywhere in this world where any flowing, streaming, was possible, the dominant flux occurred over the route of least resistance and minor simultaneous fluxes over all other routes or detours. When some faculty colleagues heard of it, they accused me of 'not teaching psychology'.

I might quote from 'The Fundamental Laws of Human Behavior' (although I published this book so late as 1911), p. 61: "We must not think of a nervous process as ever taking strictly a single path. We must, on the contrary, think of any nervous process as permeating the whole nervous system, but only along a certain path with great intensity, along all others with very small intensities. Only thus can we understand the function of the nervous system as a unitary function." I know of no psychologist (unless S. I. Franz be the

exception) who at that time taught anything similar. They all taught that a current was 'switched in the synapse' with exclusive passage either here or there, like a railway train. The number of those who still stick to the switch conception is rapidly diminishing, but not yet extinct.

Then we brought up in the classroom this question: Conceiving of learning as a variation of resistance in one of the innumerable nervous paths constituting the sensory-motor network—*what is a conceivable cause* of such a resistance change? In natural science a cause is one event which (overlappingly) precedes another event which is then called the effect. Is it not so in psychology? Let us, however, discuss and restate this question before we answer it.

I remember some ludicrous happenings which I experienced at that time outside of my classroom. A colleague wrote in a review of an article of mine: "Meyer speaks of resistances. He ought to prove, first, that the nervous system has any resistance." This reviewer could not see that to one accustomed to theoretical physics a resistance is nothing but the reciprocal of a (numerical) conductivity. One can scarcely deny that the nervous system has conductivity. How then can it fail to have resistance? Every number permits the use of its reciprocal value whenever the latter is more convenient, provided we designate it conveniently. The times have changed, and such criticisms no longer occur.

The above question which I proposed to restate, was put before my students also in these terms: Conceiving of learning as a variation in the direction 'naturally' taken by 'the' current, what can be the cause of the change of the current into a different direction? My students were sufficiently trained not to offer a metaphysical answer, such as saying: 'The cause is some will.' Nor an evasive answer such as 'something operating the switches in the synapses', for they knew too well that the nervous system has not the faintest resemblance to a switchboard (that it does resemble a fishing net crumpled up in a heap), and that the conception of switchboard operators living in the brain, which they found spooking in some widely used psychology books, was wild fancy.

They thought for a while. They sought for a possible material cause. Then one of them said: "The only cause of which I can reasonably think as playing a normal rôle in changing *one* nervous current is *another* current."

"That is the only normal cause I, too, can think of," I told him. There must be a second current. In order that there be a second current, there must be two stimuli, one for each current. So we formulated the law of learning as 'a law of double stimulation'.

We could not formulate the law of double stimulation in an equation on the blackboard or on paper because at that time, thirty years ago, the physiology of the sensory-neural-motor function was so imperfectly known, that it was risky to invent a formula. It is risky even today. Yet during the last few years the electrotechnical advances have made possible such researches as those of Adrian and Erlanger, which promise to increase considerably and rapidly the scientific knowledge in this branch of physiology.

For thirty years the law of learning has been to me 'a law of double stimulation'. I did not say much about it at first for a few years, from fear of being accused of 'not teaching psychology'. But in 1908 I published a paper in the PSYCHOLOGICAL REVIEW under the title 'The Nervous Correlate of Pleasantness and Unpleasantness' and used the opportunity to place the law of double stimulation before the public in the simplest imaginable form, that is, with the strictest *simultaneity* of the two stimulations. Let the sensory points be S_1 and S_2 of existing reflexes (or existing habits) and the motor points be M_1 and M_2. I quote from pages 302–304: "The outcome is that when some time later the sensory point S_2 is stimulated separately, the current now passes to M_1, finding here less resistance than over the 'natural' path to M_2. That is, the original reflex has been put out of function, an instinct has been modified into a habit with a different kind of reaction."

I had never heard of Pavlov's theories at that time. It is clear that *a law of learning by doubleness of stimulation* is the same as a law of learning by adding to an unconditioned stimulus a conditioning stimulus. Whether we name it by

the one or the other phrase does not change our fundamental concept of what learning, habit formation, consists in. I even stated in that article of 1908 a condition of assuring or preventing permanent success of habit formation which Pavlov, as I did not then know, calls, in its negative aspect, 'experimental extinction'. I wrote on page 304: "Of course, one condition must be fulfilled while the habit is being formed: S_2 must not be stimulated too frequently alone." If we never stimulate our conditioned reflex dog or child so as to bring about the flow of saliva with inevitableness (by giving him as a so-called reward a piece of meat), then the bell-ringing finally ceases to cause the flow of saliva already fairly established as a response to the bell. Why not simply refer to this as forgetting? Or perhaps as forgetting favored by the forming of *irrelevant* bell habits! That seems to me much more sensible than to call it by a term so queer as 'experimental extinction'.

When 'conditioned reflexes' (there seemed to be something bewitching in that name!) became a *fashionable* subject for discussion, I soon stated my conviction in our Association meetings and in publications that *all habits without exception*, when properly analyzed, were reducible to the same law of double stimulation, no matter whether we enumerated them as simple modifications of *response*, or modifications of what was adequate *stimulation*, or *selecting* a skilled reaction from a mass reaction, or learning a *vocabulary*, or learning to run a *maze*, or learning nonsense *syllables in a series*, or *memorizing* poetry or prose. But even those of my psychological brethren who were most active in making Pavlov's work familiar, strongly opposed me in this, and asserted their belief 'that many kinds of habits were of a different sort and origin'. But time seems to have decided in favor of the concept that any variation of nervous conductivity classifiable under training for skill, or under habit, or learning, or memorizing, is an exemplification of the law of double stimulation.

The process of 'double stimulation', or call it 'conditioning', of course takes time. Any event on earth takes time. It is self-evident that a biological adaptation is *more pronounced* if, instead of being adaptation to a short ex-

perience, it is to one prolonged or frequently repeated. That such an adaptation, while it is expected to become stronger gradually, ceases to be strengthened and even *reverts* when the effective cause is eliminated, is self-evident. But *therein* is not found *the law* of the natural phenomenon of adaptation. *Therein* are found only examples of arithmetic.

Before stating how in time natural things often happen to occur, *the law* will state what, *i.e.*, how many, those natural things are which *must* occur, and which a keen enough intellect (may God send him to us!) will put as *the variables* (other than *t*) into *the equation* which he will *invent*.

The 'laws' of Frequency, Duration and Recency are no scientific laws, but are childish utterances thrice variegated: (1) that the oftener any measured *times recur*, the greater is *the sum* of duration to which they add up, (2) that when time appears as a variable in an equation, the equation undergoes the quantitative development for the symbolization of which *time* has been made *one of the variables*, (3) that forgetting, unless mitigated by relearning, is *unmitigated forgetting*. Do students go to college, or do business men read books on applied psychology, in order to be told such trivialities? Is it true what a joker has said, that in psychology students learn that which they knew all the time?

When you cut a dog's tail frequently, say, an inch every day, the tail is much shorter on the fifth night than it was on the first or on the second night. When you drink a glass full of poisoned water and after five seconds vomit, you absorb much less of the poison than if you had waited five minutes before putting your finger in your throat. When you have missed four successive meals, your appetite is much more whetted than if you had missed only one meal. Here then are three more 'laws' on which it is possible to waste printer's ink. Which college departments are willing to stultify themselves by including the teaching of these laws in their courses?

Frequency, duration and recency are no laws of learning. There is only one law of learning. It is double stimulation. Secondary laws may of course be derived from that.

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EMOTION: AN EXAMPLE OF THE NEED FOR REORIENTATION IN PSYCHOLOGY

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During the past few years theories of the emotions have been subjected to much criticism and revision, and in certain instances important objections have been made to what were previously held to be almost self-evident facts. But this criticism seems to have been less far-reaching and destructive than the situation warrants. There is beginning to arise this important question: *Does the concept of emotion serve any useful purpose in scientific psychology?* The writer suggested in a previous paper (**2**) that the differentiæ of emotion are very inadequate and, indeed, incapable of exact application in distinguishing an 'emotional' from a 'mental' state.[1] It was pointed out that differentiation is customarily made, very inexactly, in terms either of the stimulus situation or of the *degree*, rather than the *kind*, of reaction. Meyer (**5**) has recently stated a similar point of view, and has predicted that 'emotion' will eventually disappear from psychology. But in neither Meyer's paper nor the writer's has the concept of emotion been sufficiently analyzed.

It is evident that contemporary investigation of the emotions has proceeded upon the following assumptions: (1) that there is an important difference in *kind* between emotional and other responses, and (2) that there is an important difference in *kind* between one emotion and another. The second of these assumptions has recently been questioned by a number of psychologists. The experiments of Sherman (**6**) and the theoretical analysis of Dashiell (1) have called attention to our dependence upon the stimulus situation for

[1] 'Mental state' should be understood to refer to intellective or cognitive processes uncomplicated by affective reactions. The term 'state', as used here and throughout the discussion, is intended to refer to a cross-section of the individual's responses at a given moment.

the name to be assigned to any given emotional manifestation. Dashiell's frequently quoted statement is that it is quite possible that "such names as are conventionally used for different emotions refer to different types of viscerally facilitated or inhibited overt *behavior* patterns, that have been classified and labeled more in terms of their *social* significance than in terms of their *visceral* components" (1, p. 325). Dunlap (3) goes a step further in making the point that the various emotions not only *are* named from the stimulus situation as apprehended, but *must* be so named, since the responses to which these names are attached do not appear to have any constant distinguishing characteristics apart from their occurrence in a certain context. He says "that in which the various 'fears' resemble one another, the various 'griefs' resemble one another, and the various 'joys' resemble one another is in the likeness of the situations in which these states arise, or rather in the perceptual and ideational apprehension of these situations" (3, p. 574). He further suggests that the attempt to classify the various emotional responses as of one kind or another is as futile as the attempt to classify the instincts. Such distinctions are based upon an interpretation of the stimulus situation rather than upon specific and differentiating reactions of the subject. But no question is raised by these writers as to the validity of the assumption that *there is an important difference in kind between emotional and other responses*. Dunlap, who most nearly approaches the questioning of this proposition, says that "Emotion, however, is a fact quite aside from questions about 'the emotions'. We do get 'stirred up', 'moved': we feel. All introspection agrees on this point" (3, p. 573).

What, then, are the accepted differentiæ of emotional states? Experimental differentiæ which are conclusive in their application have not been discovered. Pulse rate, respiration, muscular changes, galvanometric phenomena, and other measurable responses have all failed to distinguish definitely between an emotional and a non-emotional condition. Introspection alone has been able to determine the issue, and how accurately and by what criteria we can only

surmise. And even introspection has at times given an equivocal answer. For example, in certain experiments on the effect of adrenalin, some of the subjects reported feeling '*as if* afraid', etc. Indeed, in ordinary life any individual must experience many states of which it would be difficult to decide whether they were emotional or non-emotional. Our lack of success in recognizing emotion could be due to faulty experimental techniques and to inadequate guides for introspection, but it could also very well be due to the fact that the object of our search is, in the form in which we seek it, non-existent.

An examination of the various definitions of emotion offered by contemporary psychologists indicates that the differentiation of emotion from other states has been proposed on the following bases:

1. The physiological mechanisms involved in the response, *e.g.*, visceral as opposed to somatic activity; or activity of the thalamus as opposed to activity of the cerebral cortex.

2. Degree of arousal, or intensity of reaction, of the organism.

3. Disorganization, and consequent ineffectiveness, of behavior.

4. Interpretative data of various sorts, *e.g.*, descriptions of the content of consciousness, or of the kind of stimulus-response situation.

5. Various combinations of the above differentiæ.[2]

Though the term 'emotion' is assumed to denote a unique state of the organism, no one of these criteria has been successful in delimiting this state in such a way as to make it appear different in kind from other states. Each attempts to describe a certain kind of behavior or experience which occurs when the individual is subjected to a certain type of stimulation conventionally called 'emotional', but neither the response nor the stimulus situation has been successfully delimited. Sometimes the response appears to get its name

[2] These criteria of emotion are not quotations from definitions offered by contemporary psychologists but are summaries of the various bases of differentiation which have been proposed in these definitions.

from the stimulus situation, sometimes the stimulus situation from the response. But, whichever the criteria employed in recognizing emotion, the distinction can always be shown to be one of degree rather than of kind. 'Emotion' does not represent a unique state; it represents merely one end of a continuum. Or rather, it represents various ill-defined points on a number of continua, according to the definition employed. The recognition of this fact has certain important consequences which will be pointed out in a later section of the paper. But first we must substantiate our contention that each of the types of definition of emotion mentioned above fails to describe a state differing in kind from other states of the organism.

Differentiation of emotional responses on the basis of the physiological mechanisms involved in the reaction has been, perhaps, the most generally accepted form of classification. The most common variety of this form of definition assumes that emotions are primarily *visceral* reactions, or, at any rate, that in the viscera may be found that difference in *kind* of behavior which separates emotions from other states. In support of this point of view it is said that changes in visceral reactions have been shown to occur under 'emotional' stimulation. (We do not, of course, know that similar changes do not occur under other types of stimulation, *e.g.*, during mental work, during strenuous physical exercise, etc.) It appears obvious that there are visceral changes of some sort occurring in the organism at all times. Which are those particular patterns of visceral action which shall be called emotional? How does the 'stirred-upness' of digestive disorder differ from that of emotion? Which visceral contractions are emotional and which are not? The vegetative mechanism passes through various cycles of increased and diminished activity as it sustains the organism and provides the basis for the energy used in motor response. Do we have an emotion whenever increased energy is supplied for motor reaction? Or when there is a decrease in energy? If so, exactly how great an increase or decrease in energy (or in the functions coöperating to provide energy) must occur

before an emotional state is produced? Other physiological mechanisms have been described in the differentiation of emotion (vascular changes, respiration, pulse rate, etc.). In each case the questions raised above apply. The distinction is one of degree.

A slightly different type of delimitation of emotion on the basis of physiological mechanism is the description in terms of the part of the nervous system involved in its control. Emotional responses are responses controlled by the autonomic nervous system, and the 'center' for emotion is to be found in the thalamus. But unless we are agreed upon a statement of *which responses* are emotional responses, it is meaningless to ask whether these responses are always controlled by a certain section of the nervous system. On the other hand, we are at liberty arbitrarily to define as emotional those responses which are predominantly controlled by any part of the nervous system which we may select. But unless these responses have certain characteristics in common, so that they are recognizable, the classification is of no psychological value. The situation here is analogous to the situation in regard to the description of reflexes. Unless reflex responses can be shown to possess constant delimiting characteristics it is of no psychological value to call 'reflex' all those responses which are controlled by certain lower neural centers. Neurological description is not psychological description, and correlations between these two classes of phenomena can be discovered only when the phenomena in each class have themselves been clearly defined.

The second type of definition of emotions, similar in certain respects to some of the definitions of the first type, is more frankly based upon the degree of response involved. In this case the emphasis is upon the intensity of the total reaction, the degree of 'arousal', of 'reactivity', or of 'tension' of the individual as a whole. The term 'emotional' has been applied to one end of a continuum which might be said to stretch from the soundest sleep to the most frantic effort. However, it is evident that excited or 'keyed up' states occur not only during stimulation called emotional, but also during

intense mental work, and during strenuous physical effort. That there are constant and characteristic differences in the patterns of the aroused states under these various circumstances seems highly improbable. Rather it appears that the situations themselves do not show clear-cut differences, but shade into one another, and that the responses to these situations are specifically adjustive and are not subject to rigid classification into categories.

The third type of definition of emotion maintains that emotional behavior is disorganized behavior—that it is the type of behavior which results when the individual has no adaptive response in readiness. But do we find that behavior, except under emotional stimulation, is perfectly smooth and well-organized? Are there no stages of disorganization during difficult mental work or during the learning of a new muscular skill? Is fumbling and confusion not the rule, rather than the exception, when adjustments other than habitual ones are being made? Should all mental blocking be called 'emotional' blocking, and should all incoördination of the muscles be termed 'emotional'? Disorganized responses are clearly found in many situations which would not ordinarily be termed 'emotional', and well-organized responses are, in the opinion of many psychologists, frequently found in the presence of 'emotional' stimulation. Unless we wish to disregard the stimulus situation and call all disorganized response 'emotional' response, this criterion is not likely to prove helpful. But even if we should adopt this procedure we should find that we had no absolute criterion of an emotional state, for organized behavior shades into disorganized behavior, with no gap between the two. Our third criterion proves also to depend upon the *degree* of the response involved.

The fourth type of definition of emotion may be called interpretative, since it necessitates the formation of a judgment about either certain characteristics of consciousness or the meaning of a given stimulus-response situation. Definitions of this kind discover emotion when a certain type of mental attitude is present; or when consciousness has a

pleasant, or unpleasant, or confused, or otherwise described quality. When applied by strict behaviorists such definitions identify an emotion by means of an interpretation of the direction or the significance of the overt behavior of the individual. But in any case an element of interpretation is involved, and the results of no two investigators can be depended upon to be identical.

Let us, for example, ask what kind of mental attitude is an emotional attitude. The question is difficult to answer. We find that we cannot describe an emotional attitude in general unless we say that it is whatever attitude accompanies certain types of bodily responses. But in that case the definition of emotion is in terms, not of the attitude, but of other phases of behavior which have been discussed in the preceding paragraphs. If we wish to use mental attitude as a differentia, we are forced to say that an emotional attitude is any one of a large number of specific attitudes—that it is, for example, an attitude of fear, of love, or of suspicion. In other words, 'emotion' is a group name for certain conventionally defined systems of ideas. But which systems of ideas are emotional and how do they differ from systems of ideas which are non-emotional? Are interpretations of situations as being interesting, peculiar, suspicious, etc., emotional or non-emotional? We have here as much opportunity for disagreement on classification as we have in the compilation of lists of instincts, and for similar reasons.

The situation is not remedied by resort to various attributes or qualities of consciousness. Emotion cannot be clearly differentiated by defining it as a state of confused or unanalyzed awareness of how the body feels, or by defining it as involving feelings of pleasantness or unpleasantness. Here again we are dealing with criteria which must be interpreted in terms of degree. How confused must our perceptions of our bodily states be before we can be said to have an emotion? And where shall we draw the line between mild pleasantness or unpleasantness and a neutral feeling tone? Or do we indeed ever experience a completely neutral feeling tone?

The fifth type of definition of emotion requires no discussion since it is based upon a combination of two or more of the four types of differentiæ considered above. In so far as each of these differentiæ is inexact in its application, the combination will be found also to be inexact.

Since it appears that no one of the proposed definitions of emotion clearly separates emotional from non-emotional states we are led to inquire by what means the psychologist or the layman actually makes his judgment as to whether a given individual is displaying emotion. Since the opinion that emotion exists as a distinguishable state of the organism has gained such wide acceptance it would appear that there must be in common usage, whether or not included in our definitions of emotion, certain means by which an emotion can be distinguished from other states. But an analysis of the ways in which emotion is usually identified points only to the inexact use of two of the criteria mentioned above: (1) type of stimulus situation, and (2) intensity of reaction. An 'emotional' response is found when the stimulating situation is interpreted as 'emotional'. For example, in psychological experiments the reactions to sudden, loud sounds, or to tipping of the chair in which the subject sits, are 'emotional' reactions, while the reactions to a problem presented for solution are 'intellectual or cognitive' reactions. In the absence of stimuli conventionally classified as emotional, we do not usually discover an emotional response, no matter what the reactions may be. An exception to this rule is found in the case of extremely *intense* responses. The psychologist is accustomed to think of very intense reactions as emotional reactions, of highly aroused states as emotional states. He is certain that he is witnessing an emotional reaction when an individual behaves violently in a situation conventionally classified as emotional. He is confused and is likely to think of illness or derangement when an individual behaves violently under other circumstances. The deficiencies of his criteria are obvious, but nevertheless he remains certain that he can recognize an emotion, chiefly because it is not difficult to recognize one end of a continuum. When he

becomes confused in classifying a response not at the end of the continuum, he assumes that he would be able to say whether it was emotional or non-emotional if he knew more about it; or he says that such responses represent, not true emotions, but feelings or sentiments. Instead of recognizing a continuum, he makes another category.

It may appear that the question whether the term 'emotion' applies to a unique state or to one differing merely in degree from other states (and therefore not definitely distinguishable) is a trivial query and of no practical import. But the history of experimentation in this field indicates that our hypothesis that 'emotion' represents a distinct category of behavior has had far-reaching results. Guided by this hypothesis, the majority of investigations have had as their object the discovery of the unique characteristics of particular emotions or of emotional states in general. But since neither 'emotion' nor any one of the specific 'emotions' appears to have such constant distinguishing characteristics, the search has proved futile. Correlation of various physiological phenomena such as blood pressure, respiration, galvanometric phenomena, etc. with artificial psychological categories such as 'emotion' has yielded no constant results. Many investigators have, therefore, been inclined to conclude that the physiological measure in question had no psychological significance. Few have been inclined to draw the alternative conclusion that the psychological category with which they were dealing had no significance, that it needed re-formulation on a different basis. Landis (4) prematurely, I think, reached *both* of these conclusions in relation to the 'psychogalvanic reflex'. The 'reflex' had little or no psychological significance, and the various psychological categories in relation to which it had been studied had no significance. Thus he says, "This much vaunted expressive reaction finally shows itself as having only a very limited psychological meaning. I feel that this loss not only demonstrates that another 'test' has failed, but that it points to the very real necessity for psychologists to clarify their thinking with respect to the psychological categories employed in psycho-

physiological correlation" (4, p. 396). Either of these conclusions alone would have been sufficient. If the psychological categories were significant, then, obviously, the 'reflex' which showed no correlation with them was *not* significant. But if the categories, such as 'emotion', 'volition', 'ideation', etc. are, as he is inclined to believe, *not* of fundamental significance, but merely loosely descriptive, then the fact that the 'psycho-galvanic reflex' fails to correlate with them indicates nothing at all about the possible psychological significance of the 'reflex'.

The same arguments apply to other physiological measures studied in relation to the various psychological categories. Though studies of these phenomena to date have been relatively futile, there is no reason to conclude that they are without significance and abandon their investigation. It has been observed that changes in these physiological functions occur when there are changes in stimulation. The problem is to study these changes in reaction in relation to each other and to more fundamental psychological classifications which we may be able to formulate. Changes in muscular tension, for example, should be studied in their own right and not as the tension of a mental set on the one hand, and 'emotional' tension on the other. It is probable that the same degree of tension will produce the same effect upon behavior, regardless of whether the tension is 'physical', 'mental', or 'emotional' in origin. We do not need to conclude that, unless a given physiological measure reveals discontinuity between the responses to, for example, emotional stimuli and all other classes of stimuli, it is of no scientific value. We should not expect any measurable function to show meaningful correlations with descriptive terms uncritically taken over by the psychologist from the layman's vocabulary.

A glance at any text-book or treatise on psychology will convince the reader that we are still very 'literary' in our approach. The number of loose descriptive categories into which we have placed psychological phenomena is appalling. If we recognized this as simply the inevitable result of our ignorance of the fundamental nature of the phenomena with

which we are dealing, the situation would be more hopeful. Instead, we are likely to take these superficial and inexact categories seriously, and to attempt to find correlations between them and phenomena which can be measured with some degree of accuracy. Thus we not only engage in futile experimentation, but we also fail to direct our attention toward the search for general fundamental dimensions of behavior.

Further progress would be greatly accelerated by an attempt to define through experiment the simple, irreducible aspects common to all behavior. Such fundamental dimensions of behavior, when and if found, might be comparable in logical simplicity and universality to the dynamic ultimates of physical science, such as energy and velocity. They might also, like these physical ultimates, be individually measurable under all types of conditions. It is obviously impossible to propose at this time even a tentative list of the dynamic fundamentals of behavior. Their formulation awaits further experimentation and analysis. But our position may be clarified by assuming for illustrative purposes that we have in degree of excitation and degree of inhibition two such basic dimensions. We may then consider the way in which concepts of this sort could be utilized in the investigation of the dynamics of behavior. By degree of excitation we shall mean the extent to which the organism as a whole is activated or aroused, not as measured by overt behavior, but as measured by the activity of those processes which supply the energy for overt behavior. There are a number of physiological measures which may, either singly or jointly, prove to be indicators of this aspect of behavior, *e.g.*, galvanometric reactions, tonus of the muscles and, perhaps less consistently, pulse rate, respiration, etc. By degree of inhibition we shall mean, not the mere absence of overt action, but the degree to which there is restraint of the tendency to act. It is difficult to state this conception in measurable terms. It may prove to be an unserviceable abstraction, or it may prove to be definable simply as the degree to which overt behavior occurs or fails to occur under a given degree

of excitation. These concepts of excitation and inhibition could be made the basis for investigation of certain phases of the dynamics of behavior by formulating such hypotheses as the following:

Other factors remaining constant,

(1) increases in the degree of excitation tend to produce increases in the speed and force (or intensity) of the activity in progress, whether that activity be a muscular performance, a train of thought, or the expression of emotion.

(2) very high (and probably, also, very low) degrees of excitation tend to produce uncoördinated reactions—whether the reactions are muscular responses, thought processes, or the behavior in an 'emotional' situation.

(3) increases in excitation appear to make the individual more sensitive to stimulation—whether the stimulation be physical, mental, or emotional.

(4) changes in certain aspects of the stimulus situation (intensity, novelty, etc.) tend to produce changes in the degree of excitation.

(5) the effect of a given degree of excitation upon overt behavior will depend upon the ratio between excitation and inhibition. For example, a high degree of excitation accompanied by a low degree of inhibition will result in intense, uncoördinated behavior; a high degree of excitation accompanied by a high degree of inhibition will result in intense, coördinated behavior; a low degree of excitation accompanied by a low degree of inhibition will result in easy-going, coordinated behavior; and a low degree of excitation accompanied by a high degree of inhibition will result in labored, uncoördinated behavior.

The value of such an approach is that these relationships, if substantiated, would hold not simply for a single inexactly defined category of behavior, such as emotion, but for behavior in general, without regard to the kind of behavior or the kind of stimulus situation. Where other factors remain constant, the same *degree* of any one of the fundamental dimensions of behavior would produce the same result.

Moreover, our descriptions of behavior would be in terms, not of inexactly defined and unmeasurable categories, but of precisely defined and measurable dimensions, the degree of each of which could be stated in terms of some scale upon which we should agree. This is not to be taken as implying that any given dimension of behavior will vary entirely independently of other dimensions. On the contrary, we should expect varying degrees of inter-dependence among the various aspects of behavior, and our concern should be to describe the total organization or pattern as determined by the various dimensions in their relation to each other.

Abandonment of the present categories in psychology might open the way for advancement in a number of different fields of investigation. It might, for example, effect a *rapprochement* between the psychology of the emotions and the psychology of thought, now too often separated by the assumption that, while one may *affect* the other, they do not in any sense represent harmonious manifestations of the same underlying processes. With a different approach it might very well appear that what we call the 'intellectual' and what we call the 'emotional' reactions of an individual display the same general pattern. For example, it might be shown that the individual whose 'emotional' reactions are easily set off has easily set off trains of ideas; that the individual who has difficulty in coördinating his overt behavior has difficulty in organizing his thoughts; that the individual whose actions are impulsive will require long training if he is to learn to re-examine and criticize his 'bright ideas'. The study of personality traits in general would quite possibly gain new vitality from this approach, for it would involve the abandonment of the study of traits defined in literary terms (these classifications are usually neither clear nor fundamental) and the substitution of the study of individual differences in fundamental dimensions of behavior and their combination into various patterns of reaction.

Since, as psychologists repeatedly emphasize, the organism is integrated and behaves as a unit, it seems reasonable to suppose that we shall be able to discover basic dimensions

running through all behavior. If we continue to direct our attention, not toward the search for such dimensions, but toward the study of ill-chosen categories of behavior, we shall inevitably obscure the significance of much of our experimental data. The field of the 'emotions' demonstrates the confusion which may result from unwise classification. There is reason to think that psychology can develop for its purposes, in this and other fields, a more fruitful set of abstractions. When our classifications have become more fundamental from a scientific point of view, our experiments will yield a more coherent picture and our science will advance at a more rapid rate.

Summary

This paper questions the common assumption that there is an important difference in kind between 'emotional' and other responses. It points out that psychologists have proposed differentiation of emotional patterns on the basis of: (1) physiological mechanisms involved in the response, *e.g.*, activity of the viscera or of the thalamus; (2) degree of arousal or intensity of reaction of the organism; (3) disorganization, and consequent ineffectiveness, of behavior; (4) interpretative data of various sorts, *e.g.*, descriptions of the content of consciousness, or of the kind of stimulus-response situation; and (5) various combinations of the above differentiæ. An examination of these various types of definition of emotion results in the conclusion that in every case the distinction between 'emotion' and other patterns of reaction is one of degree rather than of kind. It further appears that, since the precise degree of a given kind of behavior which is to be called 'emotional' is never stated, the concept is not useful in exact psychological investigation. The attempt to correlate this vague category of behavior with various physiological changes has led to confusion. We should cease our attempt to study 'emotion' *sui generis* and should study instead the variations in certain fundamental *aspects* or *dimensions* of behavior in general as these occur under varying stimulation and as they correlate with each other. Abandonment of 'emotion' and other vague and necessarily un-

measurable categories should prepare the way for the experimental determination of simple, irreducible aspects of behavior comparable in universality to the dynamic ultimates of physical science.

REFERENCES

1. Dashiell, J. F., Are there any native emotions?, Psychol. Rev., 1928, 35, 319–327.
2. Duffy, E., The measurement of muscular tension as a technique for the study of emotional tendencies, *Amer. J. Psychol.*, 1932, 44, 146–162.
3. Dunlap, K., Are emotions teleological constructs?, *Amer. J. Psychol.*, 1932, 44, 572–576.
4. Landis, C., Psychology and the psychogalvanic reflex, Psychol. Rev., 1930, 37, 381–398.
5. Meyer, M. F., That whale among the fishes—the theory of emotions, Psychol. Rev., 1933, 40, 292–300.
6. Sherman, M., Differentiation of emotional responses in infants, *J. Comp. Psychol.*, 1927, 7, 265–284, 335–351.

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DISCUSSION

PAVLOV'S THEORY OF CONDITIONING

Pavlov's recent article, 'The reply of a physiologist to psychologists,'[1] deals with two items printed some time ago in the PSYCHOLOGICAL REVIEW, one by Lashley on 'Basic neural mechanisms in behavior,' and one by the writer, 'Conditioning as a principle of learning.' The issues raised by Pavlov deserve some further discussion because they are fundamental. My own article would have been justified if its only effect had been to persuade Pavlov to additional writing on the conditioned reflex, since by his laboratory experience he is undoubtedly its most competent exponent. However, on a number of points I remain quite unconvinced after reading his 'Reply.'

The first difference of opinion that he mentions concerns a very general issue. He says,

> The psychologist takes conditioning as a principle of learning, and accepting the principle as not subject to further analysis, not requiring ultimate investigation, he endeavors to apply it to everything and to explain all the individual features of learning as one and the same process. . . . The physiologist proceeds in quite the opposite way. At every phase of his investigation he endeavors to analyze the phenomena individually and in connection with facts, determining as much as is possible of the conditions for their existence, not trusting to mere deduction or to a single hypothesis.

This characterization is substantially correct. Pavlov has been reporting many detailed experiments with resulting generalizations as numerous as the varieties of experimental procedure. It was the writer's belief that the time had arrived when an hypothesis could be set up in order to direct experimental work. The hypothesis suggested was an old and familiar one, that the phenomena of learning, when described in terms of altered movement or secretion, may be described in terms of one principle, which was called the principle of conditioning. Its statement was this: *Stimuli acting at the time of a response tend on their recurrence to evoke that response.*

[1] I. P. Pavlov, PSYCHOL. REV., 1932, 39, 91–126. All quotations will be from this article where not otherwise indicated.

In other words, it was suggested that the outstanding characteristics of learning, which have been expressed in terms of frequency, intensity, irradiation, temporary extinction, conditioned inhibition, forgetting, forward and backward conditioning, and so on, are all derivable from a more general law, the law of simultaneous conditioning or association by contiguity in time. To this end an analysis of these various phenomena was undertaken, an analysis that was, to the writer at least, very plausible.

Pavlov's second objection concerns this analysis in so far as it applies to backward conditioning. According to him the question is: What elementary properties of the brain-mass form the basis of conditioning? Backward conditioning, practice with the conditioned stimulus following the unconditioned, has a double effect, ". . . at first, temporarily, it assists in the formation of the conditioned reflex, and then destroys it,"—becoming eventually an inhibiting stimulus.

Pavlov's explanation of this is that

> . . . the cell excited by the conditioned stimulus is inhibited or comes to an inhibited state with repeated concentration on the part of the unconditioned stimulus—and the conditioned stimulus in this way meets in its cell a permanent state of inhibition.

To this there is an objection. An understanding of the phenomenon of backward conditioning can be had only by finding the conditions under which it occurs. No properties of the brain-mass have been observed; no technique for observing states of inhibition in cells has yet been suggested. An explanation in these terms is and will remain unverifiable and entirely useless for prediction. In place of this unverifiable and useless hypothesis the writer had suggested that experimental search might disclose overlapping stimuli whose presence or absence would mark the presence or absence of backward conditioning. This would be to explain backward conditioning in terms of simultaneous conditioning. No act is instantaneous, and in backward conditioning the belated cue may accompany the later part of a sustained muscular contraction. This may be the explanation for the lessening effect of backward conditioning as the interval between cue and original stimulus is increased. In the writer's experiments with backward association[2] with human subjects the cue could be practised before or after the original stimulus with like associative strength, and the writer is convinced that backward conditioning occurs only when, and in some measure to the degree that, there are overlapping stimuli.

[2] Association as a function of time interval, PSYCHOL. REV., 1933, 40, pp. 355–367.

Concerning remote forward conditioning, or "delayed and 'trace' reflexes," the writer had evidently not made himself clear. Pavlov says,

> . . . if we grant with the author that not the bell but the centripetal flow of impulses from the motor act of listening is the true stimulus for the conditioned effect, why does that effect, in the case of delayed reflexes, nevertheless come out, not at once, but after an interval—and (furthermore) in accordance with the length of the interval between the beginning of the stimulus and the beginning of the unconditioned reflex?

For this Pavlov's explanation is two-fold.[3]

> Many cyclic phenomena take place inside the animal's body. . . . The alimentary canal is periodically filled or emptied; and, in fact, changes in practically all the component tissues and organism are capable of influencing the cerebral hemispheres. This continuous cycle of direct and indirect influences upon the nervous activity constitutes the physiological basis for the estimation of the duration of time.

This is his first suggestion. The second is as follows:

> Although prolonged for a significant length of time, the conditioned stimulus remains one and the same; but for the central nervous system (and it is especially necessary to think of the cerebral hemispheres) it is distinctly different in different periods of its course. This comes out particularly clearly with olfactory stimuli, which we sense at first very keenly, and then quickly as weaker and weaker, even if they remain objectively constant. Obviously the state of the stimulated cortical cell under the influence of an external stimulus undergoes successive changes and in the case of delayed reflexes only the state of the cell near the time of the addition of the unconditioned reflex acts as a signal for the conditioned stimulus.

The writer had made his suggestion expressly to account for the fact that the delayed reflex was elicited not at once but after an interval. The 'clock' by which this timing occurred was taken to be regular changes in the movement-produced stimuli following the cue. Pavlov's first suggestion would be in entire agreement with the writer's views if he emphasized not cyclic visceral changes but skeletal muscle changes. The visceral cycles to which Pavlov refers are more or less independent of the training situation. The skeletal muscles are in much closer touch with the external world. In practice there is no guarantee that the salivary flow would be regularly accompanied by the same phase of any visceral cycle,

[3] Conditioned reflexes, Oxford Univ. Press, 1927, p. 42.

whereas this would be possible in skeletal movements initiated by the bell. Listening, for instance, is not a sudden explosive act, but a series of acts, one movement leading to another, that to a third, and so on. Just as an orchestra player keeps time through an interval by counting or by 'beating time,' so it is possible that the timing of the delayed and trace reflexes may be accomplished by characteristic movement series. It is true that such movement series might not be regularly the same, but it is also true that this variation may account for the variations observable in the response. Pavlov cites another fact which the writer believes is aptly explained in terms of these movement-produced conditioners.

> If the conditioned reflex be formed first with the short interval of a few seconds between the beginning of the conditioned and of the unconditioned stimuli, and then suddenly that interval be increased to a few minutes—then the conditioned effect, hitherto quick to come forth, will promptly go on to gradual but utter disappearance. And then, on continuing the experiment, there appears for a considerable lapse of time a period of absence of all conditioned effects. Only then does the conditioned reflex appear anew, at first just the moment preceding the addition of the unconditioned stimulus. Thence it grows gradually and recedes somewhat from the time of appearance of the unconditioned stimulus.

The writer's suggestion is that it has been necessary to condition the reflex to a new point in the movement-produced series, and that it has been 'extinguished' as a response to the original point.

Of Pavlov's second suggestion, that a constant stimulus has diminishing effects on the central nervous system, Adrian has demonstrated instances of such sense-organ adaptation to mean the gradual cessation of afferent impulses. Changes in the cortical cells are not the occasion of sense-organ adaptation. Now the effect of cutting off afferent impulses from a sense organ is to diminish the contraction of muscles and so to change actively the movement-produced pattern of stimulation. Here is positive stimulation which may serve as a delayed conditioner and we are not forced to invoke cortical cell changes invented solely for the purpose of explaining delayed reaction. Furthermore, movement-produced stimuli are essentially observable, though with difficulty, and cortical cell changes of the sort invoked by Pavlov are not. To assert that delayed reflexes take place under certain unobservable conditions is to be safe but futile.

So much for remote conditioning. To the writer's analysis of

temporary extinction in terms of simultaneous conditioning of inhibiting responses there are offered similar objections. From Pavlov's article,

> First of all the author takes a stand against us by saying that it is not the brevity of the interval between repetitions of the non-reinforced conditioned reflexes that contributes to extinction of these reflexes, but the number of repetitions. But this is absolutely untrue.

The writer's inference was made from the following table in Conditioned Reflexes, page 53.

With an interval of 2 minutes, extinction was obtained in 15 minutes.

With an interval of 4 minutes, extinction was obtained in 20 minutes.

With an interval of 8 minutes, extinction was obtained in 54 minutes.

With an interval of 16 minutes, extinction was incomplete in 2 hours.

With an interval of 2 minutes, extinction occurred in 18 minutes.

The rate of extinction depends, according to Pavlov, on the nature of the dog, the extent to which the reflex is established, the intensity of the unconditioned reflex underlying the conditioned one which is undergoing experimental extinction, *the length of pause between successive repetitions of the stimulus without reinforcement*, and the number of times the given reflex has been subjected to extinction in the same animal. The writer would agree to all of these but the one in italics, for which he would substitute conditioned inhibition. From the table there seems to be a regular increase in the time required to extinguish with the increase of interval. But it will be noticed that application every two minutes for fifteen minutes, application every four minutes for twenty minutes, application every eight minutes for fifty-four minutes, and application every sixteen minutes for two hours (with incomplete extinction) involve respectively 7.5 applications, 5 applications, 6.75 applications, and 7.5 applications, which are certainly approximately equal. The reason why Pavlov calls the writer's suggestion 'absolutely untrue' is the new fact offered by Pavlov that one application *prolonged from three to six minutes* gives an uninterrupted extinction. To this the writer would suggest sense organ adaptation, or exhaustion of the receptor, or the emotional inhibition resulting from the withholding of food described by Winsor,[4] as the expla-

[4] A. L. Windsor, Observations on the nature and mechanism of secretory inhibition, PSYCHOL. REV., 1930, 37, 399-411.

nation, and plead that his version of Pavlov's table, while not absolutely true was approximately true and not 'absolutely untrue.'

The writer's contention is that temporary extinction differs only from conditioned inhibition in that it is inhibition conditioned on a temporary set, posture, or proprioceptive pattern. When this set is broken up by extraneous stimuli it no longer is present to act as an inhibitor. This is an hypothesis open to verification. Pavlov's objection to it is that

> . . . the most important part of these impulses (proprioceptive) proceed to the lower divisions of the brain, and . . . under usual circumstances absolutely do not make themselves known to the cerebral hemispheres but serve only for the self-regulation and greater precision of movements, such as, for example, the continuously occurring cardiac and respiratory movements.

To the writer this seems not only a rather broad statement, but a statement which would, since Pavlov believes conditioning to be the exclusive function of the hemispheres, deny the possibility of acquiring any motor skills based on the conditioning of one movement on the stimulus of another, and deny that serial motor habits conform to the law of the conditioned response.

Pavlov's own explanation of temporary extinction is that it is

> . . . the manifestation, according to law, of the most important properties of the cortical cells, as the most reactive of all cells of the organism, when they remain at work for a greater or less period of time—even if generally a short one—without a satisfying accompaniment for the fundamental innate reflexes; for, the chief physiological role of excitation of these cells is to serve as signals in place of the special stimuli of the latter reflexes. As the most reactive cells, they quickly become fatigued from work and go on not to an inactive state but to inhibition, which probably not only assists in their rest but also hastens their recovery. But when they are accompanied by unconditioned stimuli, then these stimuli . . . at once, and so to speak by way of protection, inhibit them and thus contribute to their recovery.

Since no observations have been made of states of these cells supposed to have gone 'on strike' and no observations are likely to be made, this hypothesis would appear to be not only unverifiable, but unassailable and at the same time of rather little value in the prediction of behavior.

Two more differences remain. Pavlov says,

> In regard to the fact of gradual intensification of the conditioned effect during the process of its formation, it is necessary to state that in

this case it is the gradual removal of extraneous stimuli which disturbs the formation of the reflex, and not the opposite, namely—the author's view which consists in attributing to these stimuli an ever-growing rôle in creating the conditions for the effect. During our first experiments often 50 to 100 or more repetitions of the procedure were required in order to develop a complete conditioned reflex, but now 10 to 20 times are sufficient, and often much fewer.

The writer had evidently failed to make his meaning here clear because there is on this point no difference of opinion. In fact he had suggested that perfect control of the experimental conditions, which would include the proprioceptive situation as well as the exteroceptive, might well result in a certain conditioned reflex with *one* repetition.

The final issue is this: The writer had suggested that the phenomenon of irradiation, involving the effectiveness of receptors which were near, but distinct from the practised conditioning receptors, could be explained by the fact that neighboring receptors, by virtue of their nearness, called out responses (avoidance of a touch, for instance) which were substantially identical, and that these approximately identical movements furnished the effective conditioners of the saliva flow. According to Pavlov this is an irradiation of stimulation spreading over a definite part of the cortex. Pavlov's objection to the writer's explanation is that in many cases the phenomenon is observed 'without any trace of orientation.' To this the writer suggests that in such cases it is possible that closer observation would find such a trace.

It is also possible that it would not. As was said at the beginning of this article the writer was putting forward an hypothesis which still has for him a certain plausibility. It is only observation and experiment that can say the last word. It remains his opinion that this hypothesis is at least not inconsistent with any of Pavlov's facts.

It is evident that the differences between us do not concern Pavlov's laboratory findings. These have been of immense service to psychology. Our differences concern a strong tendency found throughout Pavlov's reports to interpret the facts in terms of supposititious events and states in the cortex. These events and states are not the characteristics of nerve conduction described by such men as Adrian and directly demonstrable in the laboratory. They are, on the contrary, quite unlike any of the properties of nervous tissue that have been so directly demonstrated, and they

are of such a nature that they can not be observed by any available technique. A proper and scientific theory of learning must describe the phenomena of learning, and also describe the conditions under which these phenomena appear. Both the phenomena and the conditions determining them must be clearly defined and observable. Pavlov's work has satisfied these requirements in so far as laboratory practice is concerned, but his excursions into interpretation and theory are not so fortunate.

If further experiment should establish the law of association by contiguity in time, it will not be the first occasion in the history of science that a variegated lot of experimental results have been found on analysis to be cared for by one descriptive generalization.

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